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TRANSACTIONS FOR 1869.

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PREMIUMS FOR 1869.

At a Meeting of the Society held on February 7th, 1870, Premiums of Books were awarded to:—

E. G. BARTHOLOMEW, for his Paper on "Electric Telegraphy, irrespective of Telegraphic Apparatus."

F. W. HARTLEY, for his Paper on "The Methods employed in the Determination of the Commercial Value and the Purity of Coal Gas."

PERRY F. NURSEY, for his Paper on "English and Continental Intercommunication."

VAUGHAN PENDRED, for his Paper on "Apparatus for Measuring the Velocity of Ships."

CONTENTS.

PAGE

Inaugural Address of the President 1

Explosive Compounds for Engineering Purposes. By Perry F. Nursey 10

The Drainage and Water Supply to the Town of Fareham. By Thomas Buckham 41

Electric Telegraphy, irrespective of Telegraphic Apparatus. By E. G. Bartholomew 55

The Prevention of Leakage in Gas and Water Mains. By C. M. Barker 79

The Methods employed in the Determination of the Commercial Value and the Purity of Coal Gas. By F. W. Hartley 100

Apparatus employed for Illumination with Coal Gas. By William Sugg 134

English and Continental Intercommunication. By Perry F. Nursey 154

The Need of further Experiments on the Strength of Materials. By Charles J. Light 194

Apparatus for Measuring the Velocity of Ships. By Vaughan Pendred 215

SOCIETY OF ENGINEERS.

ESTABLISHED MAY, 1854.

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1870.

TRANSACTIONS, &c.

January 18*th*, 1869.

INAUGURAL ADDRESS.

By F. W. Bryant, President.

In taking my place for the first time as President of this Society, I thank you for the honour you have conferred on me in electing me to that office, an honour which I greatly esteem, and hope, with the assistance of the Council, and the co-operation of the members generally, that I shall be enabled to carry out my duties to the satisfaction of all, and the advancement of the Society. It is now more than ten years since I joined this Society, and I have much to thank it for in enabling me to glean a great amount of information in the various branches of the engineering profession, which I could not have obtained by my own practice alone. Therefore I consider a Society like this is of great value to a young engineer, and in many cases to those who are seniors in the profession. If a member attends regularly at our meetings, as he ought if he possibly can, I do not believe there is any paper read, or lecture given, from which he may not obtain some advantage, either from the paper itself, or by its causing him to think on the subject on which the paper is written. Then there are our visits of inspection to the important works in and around London, which, I trust, may be extended to more distant localities, as occasion affords. These visits, excellent in purpose and effect, are most valuable to all the members who attend on those occasions; for it is not only from his own observations, but from the remarks of, and discussions with, those present at the time on the works actually in progress, that he who seeks to learn has every opportunity of so doing. One of our older institutions, which holds periodical meetings at the large manufacturing towns, is an instance of

the advantage of these visits, as there is no doubt that the large increase of its members is mainly due to this arrangement. I have much pleasure in remarking the kindness and courtesy with which this Society has always been received at the various works it has visited from time to time. I am happy to say I can congratulate you on the general advancement of the Society, both in numbers and position. When I first joined the Society we numbered but a hundred and twenty-seven, and now the number of its members and associates is quadrupled.

We have, as honorary members, many of those who rank amongst the highest scientific men in the world; and as regards the members and associates of our Society, we have representatives of every branch of the engineering profession. There is scarcely any part of the world where engineering works are being carried out, where there are not some of them to be found, working either as principals or assistants.

It is true that since the last financial panic there has been considerable depression in our profession, owing to the want of confidence engendered by over-speculation; but that cannot certainly be the fault of engineers. Doubtless it is the duty of an engineer to carry out works as economically as possible; but there is another duty of which he should be most careful, and that is not to under-estimate the value of any work, as that will inevitably lead to confusion.

Difficulties occur both at home and abroad, which one could scarcely expect to have been foreseen; for after all an engineer is but a human being, though sometimes it seems as if he were expected to see beyond "mortal ken." I hope and believe that this depression will be gradually removed, and that we shall see engineering improvements increasing here and abroad, to the advancement of our profession and the benefit of mankind. I say to the benefit of mankind; for is it not to the drainage engineer that we owe the health of towns and agricultural improvements; to the railway engineer quick transit, the removal of goods, and the opening up and civilisation of whole districts; to the dock and harbour engineer the safety of the Navies of the world, and so to all the branches of our profession, which tend directly or indirectly to the good of mankind in general?

Although there has been this depression, and men say "there is nothing more to be done in England," surely this must be wrong. Are all our towns properly drained? all our rivers bridged over? all the docks, harbours, piers, and lighthouses, defences of shores (much wanted in many places), all water supplies and irrigations completed? all connecting lines of railways made? Are even the existing main lines perfect? Recent accidents lead us to think that it will be necessary to have

separate lines for passenger and goods traffic—in fact, some engineers are taking steps to secure the necessary ground. Is the utilisation of sewage properly effected? Surely, then, with the return of public confidence in properly matured schemes, we may hope that the engineers of Great Britain may find that there is yet something to be done in their native country.

In and around London of late years there have been many great and remarkable works executed, some not yet completed, and I can scarcely imagine a better school for engineers, if they have properly studied the different important engineering operations that have been carried out. Many of these works will mark the present era of engineering for generations to come. Take, for instance, that grand work the Thames Embankment, although not yet completed, the portion now opened shows the great improvement it will be to London. I hope to see it extended on both sides of the river, so that the improvement may be made complete in the metropolitan district. The main drainage works are in full progress, and in a comparatively short time we shall doubtless have London perfectly drained, and, as a natural result of this, the health of the inhabitants benefitted. The River Thames has decidedly improved in purity during the last few years, as I have had good opportunity of knowing. Various kinds of fish, from the dace to the porpoise, certainly made their appearance during last summer in the neighbourhood of Blackfriars Bridge. Then we have the Metropolitan Outer and Inner Circle Railway; although not quite completed, yet the work already done is sufficient to command the admiration of all observers, professional or non-professional.

Such a varied practice cannot have occurred in any other railway I know of, and it is, I think, a good example, showing that, whatever difficulties arise in the construction of works, they can be successfully met by a good engineer. Then there is the East London Railway in course of construction, which will no doubt be of great service for public traffic when completed; also other small branches, which are of importance to the main lines they belong to, and all tending to complete the Railway System. We have also had new docks—I allude to the Millwall Docks—opened during the last year, and improvements and enlargements at the West India Docks, which will no doubt be appreciated by the mercantile communities of London, and I may say of the world. Good dock accommodation is a great desideratum, though sadly neglected in places where it is greatly needed. There are many places on the coasts of England where good docks would be of great value and convenience, and I hope to live to see many positions naturally formed for harbours and docks taken advantage of for their construction. The bridge

over the Thames at Blackfriars has made good progress during the last year, and I expect to see it open for public traffic about the middle of this year. This will also be a great metropolitan improvement. The Holborn Valley Viaduct will be of great utility, and is also progressing rapidly towards completion. There is an improvement very much wanted in London, and that is a footway bridge crossing over our principal thoroughfares; and in places like Regent-circus or the bottom of Fleet-street a four-way bridge is very desirable. I see no reason why they should not be tried. A light iron structure might be erected at a trifling cost, and it would very soon be proved if it were of practical use. Engineering work in Great Britain has generally been very slack during the last year. It has had many things to diminish its chance of prosperity. The Abyssinian war, the unsettled state of, and revolution in, Spain, and the doubtful position of politics with regard to war in Europe, all tended to the stagnation of work. For the prosperity of civil engineering peace is absolutely necessary, and I hope and think we may look forward to it with confidence.

The general election, without doubt, materially interfered with engineering establishments all over the kingdom. I think, however, I may congratulate you on the fact, that the prospect of an increase of work is decidedly brightening in the iron districts, which is a good criterion for our profession generally, although report states that Belgian houses have an immense amount of work in hand. The *Iron Trade Review* of December 23, 1868, states that they "have now secured, or are in treaty for, contracts to the extent of something like 90,000 tons of rails;" and further on it remarks: "We may ask, what English firms durst venture to take contracts for such a period in advance in the present state of the market, in consequence of the constant insecurity from a movement on the part of workmen for advanced wages; and experience has shown that this is almost sure to be their tactics whenever they know that employers have heavy contracts on hand." This, I am sorry to say, I believe to be the case. Every man has a right to obtain as much as he can for his labour; but it is unfair, after a master has taken a contract, for workmen to demand a rise of wages before the completion of that contract, and it is much to be regretted that workmen should do so, as in many cases it prevents the ironmasters of England from taking work. It is a difficult question, but one of great importance, to institute a proper system of arbitration between men and masters; and whoever could arrange a good method of effecti[illegible] would be rendering his country a great service. The [illegible] manufactured iron into France has decreased by s[illegible]ns in 1868 from that in the preceding

year, still I perceive a great probability that the decrease will be altered to an increase during this year. I am glad to find, also, from public reports that our exports of iron to America are decidedly on the increase; and from the friendly relations now existing between the two countries, and also from the low state of stocks of iron generally, we have every reason to expect that our exports will be much larger during the present year, and I most sincerely trust that not only mutual improvement of commerce, but of good feeling, will continue between Great Britain and America and all other parts of the world. I am certain with regard to our nearest neighbours, that the sentiment of peace which the Emperor has lately expressed, confirms my idea of the probable increase of trade generally.

While on the subject of iron, I have great pleasure in remarking that we have to welcome a new society lately formed, under the title of the Iron and Steel Institute, and I learn that in the short space of two months, a hundred and fifty gentlemen directly connected with the iron and steel interest have signified their intention of joining the new institute. There can be no doubt that much good will result from the information they will be able to afford our profession. In Great Britain there have not been very many engineering works of great interest carried out during the last year. The principal amongst those completed is the bridge over the Mersey at Runcorn, and the viaduct over the Solway. Of those in progress there is a bridge over the Trent at Nottingham, which I believe will be a very excellent example of its kind when finished. I may also mention the Chatham Docks. These works are on an extensive scale, and will no doubt be of great value to the service for which they are required. Then there is the new dock at Hull, the Sunderland Docks, and the Tyne improvements. One of the most important questions that engineers have before them is that of town drainage and irrigation—a subject that has been particularly brought before us by our late President, who certainly has given us the benefit of his considerable practice in that branch of civil engineering, and for which we have much to thank him. The boldest of the proposed engineering works connected with England is that of passing under or over the Channel, for which we have three schemes, *i.e.*, by means of a bridge, a tunnel, or a steam ferry. The first of these I do not expect to see carried out; the second I believe to be quite practicable, the cost appearing the great objection; the third seems to me the most practicable proposition, and I have no doubt would be found of great service to the public. On the Continent, during last year, there have been some remarkable works completed, and many in progress, and if peace continues,

and Spain settles down to working instead of fighting, I think we may reasonably expect a considerable advance in engineering works in that country, where there is a considerable field for engineering of all kinds; in Turkey, also (railways more particularly), improvements are much wanted. Then there are the Wallachian lines to be completed, and I understand some large contracts have lately been taken in that part of the world by English contractors. This year, in Russia, there will be heavy railway works in progress, whilst Italy and Sicily will possibly add something to the European railway system. The most remarkable railway work opened for traffic during the past year is that over Mont Cenis, and, in spite of differences of opinion as to details in working, it must be considered a great engineering achievement. Its rival, the Mont Cenis tunnel, is gradually progressing towards completion, and when it is done will solve the problem of under *versus* over mountain travelling. Bridge engineering has also been represented by some very fine and, in some cases, novel structures. Perhaps the most novel, and certainly one of the finest, is that over the Moldau at Prague, on the rigid suspension principle, which seems to be very successful; and I am very pleased to remark that Mr. Ordish, the engineer of the bridge, an old member of this society, and one of its past presidents, has been presented with the gold medal of Arts and Sciences of Austria, by the Emperor of that country on the opening of the bridge, and one of our prizemen (Mr. Wessely) with the gold order of merit for the part he has taken in the construction of the bridge.

One of the finest railways bridges in Europe has been constructed over the Rhine at Kuilemburg, in Holland, which does great credit to its engineer and those who have carried it out. Its construction is, doubtless, familiar to you all, through the medium of the London scientific papers. It is remarkable for its large span of 492 ft. in the clear. Among great European works in progress, may be mentioned the docks at Havre, which will, I understand, be very fine specimens of dock work when completed; the drainage works and canals in Holland, and other works in progress in Western Europe.

Let us now travel eastward, and crossing into Africa, we come to one of the grandest works of modern times—the great ship canal across the Isthmus of Suez. Already a small schooner, *La Levrette*, has made a passage by the canal across the isthmus; and I learn on the best authority, that in about six months it is probable vessels of the largest size will be able to pass through the isthmus from sea to sea. When this is realised, what a great engineering triumph it will be; perhaps even altering the fortunes of nations. Certainly it will make

a great difference in our Indian trade, but time will soon show; and it is scarcely worth theorising when we shall so shortly know what the result will be. In India, again, what a revolution engineering is effecting in that vast empire, and what an immense amount of work there is yet to be done in every branch of our profession; from what I can learn, there is now a great want of engineers in India, preventing works already proposed being carried out. There are many difficult problems in engineering to be solved: one, I may mention, that appears to be more than ordinarily so, and which has yet to be overcome. I allude to the bridge over the Hooghly, which has taken up the attention of many engineers for some time past, without any practical result. Still there are works progressing of very great extent, in which there are many difficulties, taxing the skill of the engineers in every way. Take, for instance, the Great Indian Peninsular, the Bombay and Baroda, the Great Southern of India, and other railways, the dock and harbour works at Bombay and other parts of India, the progress of which we have reported to us from time to time.

Telegraphic engineering is extending most rapidly, and ere the end of this year we may expect that the French Atlantic cable will be laid, forming another link between the old world and the new. And I believe that the time is not far distant when we shall have communication by telegraph with China and Australia. One seems to suggest the other, as it would doubtless be *viâ* China that a telegraphic line to Australia would be laid. Then there are the North Sea cables, which will be connected with an overland cable and carried on to India through Prussia, Russia, and Persia. That which more particularly concerns us here is that our government will in all probability arrange that the Postmaster-General shall have complete command of the telegraphs in this kingdom, which will be of great importance and value to the public in general, and I hope to see before long telegraphy as cheap as postage.

Engineering goes hand in hand with civilisation, and, as we have seen, the rapid alteration railways have made in changing the aspect of towns and villages in England, and even the manners and customs of their inhabitants in many ways, so can we imagine the great alterations that will take place in India when railways become common and good roads and bridges abound in every locality. These alterations will probably extend to China, and that curious race, as they become more civilised, will be better known to us, and their barbarous customs discontinued.

In Australia also engineering is progressing. There are even now many large works requisite for the benefit of its inhabitants

generally, and which as the population increases will become absolute necessities.

In America, since the late sad warfare has ended, engineering has made a brisk recommencement in many places, and I learn on good authority that the great question of the junction of the Atlantic and Pacific Oceans by a ship canal across the Isthmus of Darien is again mooted, and that the American government have actually sent a gentleman of high position to make negotiations and inquiries respecting its being carried out. Our transactions for last year will contain, in papers read by Dr. Cullen, an excellent account of all that has been done with regard to this grand project.

In railway and bridge works American engineers have proved themselves remarkable for their boldness of design and rapidity of execution. Take, for instance, the suspension bridge thrown across the Niagara River last year, which has the tremendous span of upwards of 1260 ft., or nearly double the width of the centre span of the late Hungerford Suspension Bridge. And we read of still greater spans as being proposed, and which will probably be carried out. Then there is the proposed bridge over the Mississippi at St. Louis, which will be a novelty in bridge building, as it will consist of three arches constructed in steel, the centre span of which will be 515 ft., or some 35 ft. wider than twice the span of the centre arch of Southwark Bridge. Some of the railways have been constructed with great rapidity (although it must be borne in mind that the style of work is totally different to that prevailing in England), and great ingenuity has been shown by the manner in which American engineers have carried their railways through most difficult countries. Both in North and South America there is a vast field for engineering enterprise in all its branches. Also in Canada it is known there are many important works required that sooner or later must be carried out.

I have not perceived anything particularly novel that has been introduced into civil engineering of late, although I must not forget to mention the fact that, instead of building docks abroad, they are built at home, and sent abroad to be placed in position. I instance the Bermuda floating dock, built in the Thames, and now lying in the Medway, which will leave for its destination early this year, also a floating dock for Bombay.

In foundations—which are the most important studies of an engineer—cylinders and screw-piles seem to be the favourite methods at present for bridge foundations, piers, and other works in water. In the London district it seems the general opinion that, for large buildings, it is necessary to go down to the London clay fo dation. I cannot perceive the absolute necessity

for this, and I think in many cases it might be avoided, at a great saving in cost. In mechanical engineering improvements in detail are constantly being made.

The great question as to the manufacture of steel seems to be still in dispute. We have not seen it used for any structure of importance in England at present, although I think it is probable we may do so before long. In cases where lightness is required with strength it might be used very advantageously.

Hydraulic engineering has advanced to great perfection. Photography, which has been much improved during the last year, has proved invaluable to the engineer in illustrating the progress of works. Electricity—the most wonderful of all things applied for the use of mankind—has not yet been successfully used as a motive power; a single idea, however, might revolutionise the whole of our present practice. Civil engineering is the application of natural philosophy, and it is by studying the great laws of nature that we may hope, no matter by how small advances, to improve our profession.

In conclusion, let me particularly urge upon you, the members and associates of this Society, the necessity of earnestly working for its advancement. By becoming members, you show that it has your good wishes; but it wants more than good wishes, it wants your working assistance to further its objects. By attending its meetings you show a disposition to support those who are working for it; and by contributing papers, a willingness to give the knowledge you possess, not only to the Society, but to the profession generally.

February 1*st*, 1869.

F. W. BRYANT, PRESIDENT, IN THE CHAIR.

EXPLOSIVE COMPOUNDS FOR ENGINEERING PURPOSES.

BY PERRY F. NURSEY.

ENGINEERING operations have from the earliest period included the removal of large masses of rock and other similar natural formations, and the object has ever been to effect their displacement as quickly, safely, and cheaply as possible. In the olden times the work of removing these obstructives was tediously slow, but the invention of gunpowder greatly facilitated such operations. In later years the development of chemical science has added a number of explosive compounds to the scanty list of those at the command of the ancients, and in the present day we have many agents other than gunpowder, and much more powerful, by which blasting and quarrying may be effected. The objects in view in producing a substance to supersede gunpowder, are—or, at any rate, should be—to obtain a material which shall possess a more violently explosive action; which shall either cost less, or, what is the same thing, do an equal or greater amount of work with a lesser quantity, and, above all, which shall be non-explosive except under the actual conditions of work. This latter quality is, perhaps, the most important, and has been rendered so by the innumerable accidents which have happened with gunpowder either when in store or at the time of use. The purpose of the present paper is to notice what has [illegible]e towards superseding gunpowder, and to indicate the [illegible]s which practical experience points out [illegible]s being th[illegible]e its place in engineering operations [illegible]quiring i[illegible]

Although many attempts have been made to supersede gunpowder, but few have practically succeeded, and this arises not so much from any inadequacy on the part of the substitutes, as regards power, but on account of the extreme liability of most of them to premature explosion from varying causes. Gunpowder itself is open to this objection, and hence the propositions to reduce the risk by mixing it with protecting ingredients. But this is not enough, we must go a step further. What is required is a material over which we can have perfect command, one which shall do no more than burn when in contact with air, but which shall equal, if not exceed, gunpowder in its power when ignited in an air-tight chamber, as in a bore-hole, or the barrel of a gun. The necessity for this is evidenced almost daily in one or other of our mining districts, where a large percentage of the explosions occur in the blasting operations. How frequently is gunpowder ignited by stray sparks, even when standing about, but much more frequently do accidents arise when tamping is going on. Here the contact of the metal rod with the rock leads to many a fearful accident. So much is this so, that the Royal Cornwall Polytechnic Society have taken the matter up, and have suggested safe methods of performing this dangerous operation. But however careful a miner may be, there never can be perfect immunity whilst he has to deal with a material which carries within itself all the elements of danger and destruction. To meet the case a perfectly inexplosive material is required, one which will not explode so long as the atmosphere has access to it, but in which all the active energy of gunpowder is developed immediately it is fired out of contact with the air.

Gunpowder itself is at present more largely used than any other explosive material, and it is a remarkable fact that notwithstanding the centuries which have elapsed since its first discovery, no radical or permanent change has been effected in its composition. Slight variations, it is true, have been made from time to time in the proportion of its constituents, but, in the main, gunpowder remains much as it was six hundred years ago. But the danger ever present in handling this material has always been so patent that many years since means were devised for rendering it harmless while in store, and restoring to it its power at the time of use. Colonel Ryley was the first to propound this theory, and he submitted his plans for enveloping the grains of gunpowder in bone-dust, to the government some five-and-twenty years since. In later times—in fact, very recently—Mr. Gale's proposition to render gunpowder non-explosive and explosive at will has been much before the public. His plan was to mix ground glass with the powder for storage and trans-

port, and to sift it from it again when it was required for use. This addition of a large amount of a foreign substance with the powder no doubt answers the purpose most effectually: but unfortunately there are practical difficulties in the way of its adoption. The objections are, increased bulk and weight for transport, the necessity of numerous sets of mixing and sifting apparatus, and the utter impropriety of having to prepare an explosive material just when it is required for use. Besides, in blasting operations, the accidents usually occur in charging the mine, therefore a system of this kind would here be of no value whatever.

Before quitting the subject of gunpowder, it may be interesting to notice the force this material is capable of exerting when used for blasting purposes. The following particulars show the amount of earth or rock thrown down or removed by 1 lb. of powder under various circumstances, the results being taken from actual practice. At the round cliff, Dover, 85,232 lb. of chalk were thrown down by 1 lb. of powder. In the Leith cutting, Tunbridge, 31,860 lb. of hard white sand were moved by the same weight of powder. At Plymouth, 22,000 lb. of limestone were moved per pound; in small charges only 8900 lb. were moved. In Antrim, 45,084 lb. of white limestone, and 32,430 lb. of winstone or basalt were moved by 1 lb. of powder. At East Dunmore, 14,280 lb. of hard conglomerate were moved; and on the Londonderry and Coleraine Railway, 22,400 lb. were thrown down by 1 lb. of powder. Taking the mean of these results, we have 32,832 lb. of material to 1 lb. of powder.

Numerous compounds have been brought forward from time to time, for which it has been claimed that they perfectly superseded gunpowder. But, until very recently, no material has been found which would answer all the practical purposes, and fulfil perfectly all the conditions and requirements of that most important material. Saltpetre is the agent to which the characteristics of gunpowder, as an explosive material of permanent character, are mainly due. It is to the substitution of other nitrates for this constituent that most attention has been given, and the nitrates of sodium, lead, and barium have been successively tried. But although the products, which have been known by the names of soda-gunpowder and barytic-powder, &c., have obtained a certain amount of temporary success, they have ultimately been abandoned. In fact, all mixtures of this class, when compared with gunpowder proper, have been found to exhibit important and radical defects. Chlorate of potash has been a favour[illegible] [illegible]bstance with inventors, notwithstanding its violently e[illegible] [illegible]ature. The object has of course been to

tone down its violence by proper admixture with other ingredients, and the resulting products have been to some extent successful. One of the earliest mixtures of this class was German or white gunpowder, which was tried, but proved unsuccessful. Many preparations of a similar character have also been brought before the public. Of this class is Ehrhardt's powder, the invention of which is also claimed by Mr. Horsley. M. Ehrhardt's compositions are as follows:

Blasting Powder.

Chlorate of potash	½ part
Nitrate of potash	½ „
Tannin or cachou	1 „
Charcoal	2 „

Powder for Artillery.

Chlorate of potash	1 part
Nitrate of potash	1 „
Tannin	1 „

Powder for Shells.

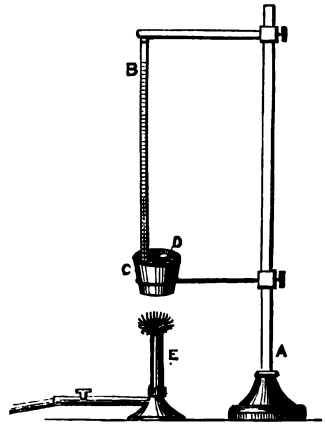
Chlorate of potash	1 part
Tannin	1 „

Mr. Horsley's powder is a compound of chlorate of potash and gall-nuts in proportion by weight of three to one. The ingredients are ground separately to a state of fine powder, and then passed, also separately, through a very fine wire sieve. The two ingredients so prepared and thoroughly dried are blended when required to form the explosive compound. The blending of the ingredients is safely and easily accomplished by passing them in a mixed state through a series of horsehair sieves arranged one below the other, and set in motion. Upon the upper sieve the two ingredients are first mixed by being run together from two receptacles placed above the sieve, one containing a given weight of chlorate of potash, and the other one-third of such weight of gall-nuts. As the chlorate of potash is much heavier than the gall-nuts, the volumes or measures of the two receptacles are about equal. Motion being imparted to the sieves, and as the two finely ground ingredients pass downwards through the sieves, they become blended, and form the explosive compound. Powders in which chlorate of potash is an ingredient

are undoubtedly somewhat dangerous. The fact, however, of cannon priming tubes, which are composed of chlorate of potash and ter-sulphide of antimony, having been prepared, stored, and used for more than thirty years past without accident, ought to relieve apprehension on that score. When treated as it should be, with care, and not improperly blended with combustibles, chlorate of potash is practically safe. With regard to the explosive powers of Horsley's powder, it may here be interesting to adduce a few facts in the shape of results of trials which came under the author's notice, and which were made to institute a comparison of its strength as against gunpowder. An eprouvette, weighing with its carriage 10 lb. 2 oz., was placed on a fir plank in a perfectly level position. The charge in each instance consisted of 50 grains of the various powders, and was kept in place by a small wad of thin paper. The recoil of the eprouvette, when charged with fine grain sporting powder, was $\frac{9}{16}$ in.; with very fine grain sporting powder, $\frac{11}{16}$ in. Fine grain sporting powder in a state of meal, and compressed by a weight of 400 lb. on the square inch, gave a recoil of $4\frac{2}{16}$ in. Horsley's powder in a similar state of meal, and with a similar pressure of 400 lb. per square inch, showed a recoil of no less than $11\frac{9}{16}$ in. These results afford some idea of the relative power of Horsley's powder and the best gunpowder. The author has examined some blocks of elm which had been submitted to experiment to show the comparative disruptive force of Horsley's powder and of common gunpowder. In each case equal charges were used, and the eprouvette was discharged one inch from the wood and at right angles to its face. The disruptive force of Horsley's powder on the wood was as if a solid body had been driven into it, separating the fibres and tearing a hole completely into it. The force of the small grain best sporting powder merely left a mark upon the surface of the blocks.

And here the author would slightly digress to say a few words upon the ignition point of explosives. Although the temperature at which explosive compounds will ignite is an important element in the investigation of their character, the author is not aware that any but the rudest means have hitherto been adopted to ascertain their ignition point. In one instance which has come under his notice, these means consist simply in placing the powder to be tried in one of a pair of gun barrels, a thermometer being inserted in the other. On the application of the required heat the powder ignited, and the temperature was registered by the thermometer in the other barrel. But this plan is open to objection on account of several defects, which will be apparent from the comparatively rude nature of the test. These

defects, however, are entirely absent from the apparatus shown in the annexed woodcut, and which was devised by Mr. Horsley, for ascertaining the ignition point of explosives. This it does with the greatest exactness in all cases, giving the most satisfactory results. It consists of a stand A, from which is suspended by the upper arm a thermometer B, which is graduated to 650°. This thermometer dips into the oil bath C, which is held by the lower arm. D is a small cup, which floats in the oil, and in which the explosive is placed. The oil is heated by the flame from the gas jet E, which is furnished with a stop-cock for regulating the flame.



As soon as the temperature of the oil reaches the ignition point of the powder the latter explodes, the exact degree being correctly registered by the thermometer. By this apparatus, Mr. Horsley has ascertained the ignition point of various explosives, and the following are among some of his results: Gunpowder ignites at a temperature of 600° Fahr. A sample of Mr. Horsley's powder gave 430° as the ignition point. Gun-cotton of powerful character, prepared by Mr. Horsley, ignited at 325°, whilst some of Prentice's sporting gun-cotton exploded at 410°. Trials of Schultze's sporting powder gave 385° as the ignition point. It is as well, at a time like the present, when new explosive compounds are constantly being brought under notice, that experimenters should know the character of the material they are dealing with, and which they will be enabled to ascertain by means of the above simple apparatus.

Another, and perhaps safer, application of chlorate of potash to the purpose in question was made some nine years since by M. Hochstädter, a German chemist. Unsized paper was thoroughly soaked in, and coated with, a thin paste consisting of chlorate of potash, finely divided charcoal, a small quantity of sulphide of antimony, and a little starch, gum, or some similar binding material, water being used as the solvent and mixing agent. The paper was rolled up very compactly and dried in that form. In this manner, very firm rolls of an explosive material are obtained, which burns with considerable violence in open air, and the propelling effect of which, in small arms, has occasionally been found greater than that of a corresponding charge of rifle

powder. Moreover, the material, if submitted in small portions to violent percussion, exhibits but little tendency to detonation. But as no reliance can be placed on a sufficient uniformity of action, in a fire-arm, of these explosive rolls, this alone sufficed to prevent their competing with gunpowder. The same description of explosive preparation, differing only from that of M. Hochstädter in a trifling modification of its composition, was again brought before the public in this country in the early part of 1866, having been patented by M. Reichen. The author has used this gun-paper with very good results in rifle shooting, but nothing practical appears to have been done with the material.

The mixture previously referred to as German or white gunpowder, consists of chlorate of potash, ferrocyanide of potassium, and sugar. Many years since it was proposed and tried without success as a substitute for gunpowder. Since then various preparations of similar character have been suggested for employment, either as blasting and mining agents, or for use in shells, or even for all the purposes to which gunpowder is applied. The most recent of these mixtures, with which the author is acquainted, is a white gunpowder made by Mr. H. W. Reveley, of Reading. This mixture is a perfectly white impalpable powder resembling flour, powdered chalk, or magnesia in appearance. Mr. Reveley recently informed the author that he has constantly made and used it, in preference to the ordinary gunpowder, both on account of its superior propelling power—which is at least one-third greater—and its perfect cleanliness. It produces neither smoke nor flash of flame at the muzzle on discharge, and can be used in a casemate with perfect comfort to the gunners. Mr. Reveley has used it for every purpose to which ordinary gunpowder is applicable, and invariably with the most perfect success. He has made many parcels of the white gunpowder during the last ten years, and has always found them uniform both as regards strength and other properties, and has never met with the slightest accident, although he has tested it very severely. The composition of white gunpowder is as follows:

Chlorate of potash	48	parts
Yellow prussiate ditto	29	„
Finest loaf sugar	23	„
Parts by weight	100	„

In manufacturing this powder the yellow prussiate is dried in an iron ladle until it is as white as the chlorate. The ingredients are ground separately to very fine powder, and are then mixed by means of a conical sieve until they are thoroughly in-

corporated, but not by trituration. For small quantities Mr. Reveley uses a common Wedgewood mortar and pestle, which must be perfectly dry and clean. The operation does not take many minutes, and with the above precautions its manufacture is free from danger. In loading it is treated in the same way as ordinary gunpowder, being pressed down by hand solid, but not hard. The charge is ignited in the usual way, with a common cap and nipple. In actual use it does not appear to possess a bursting so much as a propulsive power, and Mr. Reveley has obtained some of the highest penetrative results in his rifle practice with it. The economy of this powder will at once be apparent when it is stated that its wholesale cost is about 86s. per cwt., but as its strength is at least one-third greater than that of ordinary powder, its cost may be comparatively estimated at about 60s. per cwt. One important feature in the manufacture of white gunpowder is that it does not require to be—indeed, it cannot be—granulated, which process is the great source of danger in powder-mills. The universal use of the cartridge entirely obviates any objection that may be made to white gunpowder on that score, or on the score of similarity in appearance to other substances, and owing to its compact form, it only occupies half the usual space. Besides the foregoing there have been several cruder applications of chlorate of potash in the production of explosive compounds, which it is unnecessary here to notice more particularly.

Amongst other materials, wood has been pressed into service to aid in superseding gunpowder as a practical explosive. Soon after Schönbein's discovery of gun-cotton, a Prussian artillery officer, Captain Schultze, whilst investigating the subject, conceived that finely divided wood could be converted into a controllable explosive agent more readily than cotton. He produced the substance known as gun-sawdust, the explosive properties of which are mainly due to its impregnation with a large proportion of an oxidising agent. In preparing the gun-sawdust the wood is purified from all resinous substances, and is digested in a mixture of sulphuric and nitric acids. This gives a very feeble explosive material, which is further strengthened for ultimate use by impregnation with nitrates, by which it is made to acquire great explosive power. Here, then, is a powder which may be preserved in a comparatively harmless condition until required for use, when it may be rendered powerfully explosive by impregnation with the nitrates. Although its properties are somewhat similar to those of gun-cutton, many of the advantages of which it possesses, it is open to one very fatal objection. To be within the limits of safety, the completion of its manufacture must be delayed until the moment it is required for use; and,

moreover, the final ingredients are the most dangerous, and require refined manipulation. It is needless to point out how incompatible the conducting this completing process is with the ordinary details of mining; the care and nicety required in such a chemical operation must be referred to the skilled operator, and not trusted to the rough-and-ready hand of the miner. Practical safety can only be attained by an explosive agent into which the stray spark may fall without producing more than a gush of flame, a gradual burning, or without causing ignition at all, but which, nevertheless, when properly rammed home and tamped, may be fired with results at least equal if not superior to ordinary gunpowder.

During the year 1866 a new kind of blasting powder, which promised to supersede gunpowder in mining operations, was introduced to public notice in England. This was the invention of M. Gustave Adolph Neumeyer, of Taucha, Saxony, and to which the term "inexplosive" may appropriately be applied, inasmuch as there is no possibility of its exploding either during its manufacture, storage, or manipulation. Not until the proper moment of ignition arrives, when it is well rammed home and prepared to do its work, is its energy developed. Then, and only then, it manifests a power, when used weight for weight, considerably in excess of that possessed by gunpowder. M. Neumeyer, all his life connected with the management of quarries, and himself the possessor of a quarry near Taucha, had his attention forcibly drawn to the distressing accidents which are of such frequent occurrence in blasting operations, and he conceived the idea of producing a blasting powder which should combine the desired degree of strength, with perfect safety when in work. After a long series of trials and experiments, he succeeded in effecting his object by the invention of a powder which unites in itself the above important qualities. Within two years from the date of his discovery, M. Neumeyer was manufacturing this powder on a large scale, extensive mills with steam power having been erected for its production in the city of Altenburg, and in two other places in Germany.

Although Neumeyer's powder differs in colour as well as in action from gunpowder, in that it is slow burning instead of violently explosive when in contact with air, it is composed of precisely the same materials as ordinary gunpowder. To these no other substances are added, the whole secret of the extraordinary result arising simply from the method of proportioning and compounding the ingredients. A reduction is made in the amount of sulphur employed, by which means a much smaller quantity of the noxious vapours is evolved on its ignition than is produced by the combustion of ordinary gunpowder: a point of

great importance in underground mining operations. Some difference is made in its preparation, according to the use for which it is required, whether for military or for mining purposes. As a consequence, there results in the former case a powder which, when hermetically confined, explodes at the same temperature as ordinary gunpowder, whilst when prepared and charged for blasting purposes it requires a somewhat higher temperature. This, so far from being objectionable, is positively advantageous, inasmuch as it makes the possibility of accidental ignition more remote. Bickford's safety fuse, which is now so extensively used in our own and continental mines, is best adapted for the ignition of this powder. Another important feature in Neumeyer's powder is that, although no coating or glaze is imparted to it in manufacture, it is not more hygrometric than ordinary gunpowder, whilst if wetted and dried it is said to retain all its good qualities in full force. Ordinary powder is more powerful as the size of the grain is increased, but Neumeyer's powder when in a condition of fine dust is equally if not more efficient than the other. From what has been said, it will be seen that the new gunpowder embodies safety in manufacture, in transport, and in handling, preparatory to actual use, whilst it has been proved to be superior to ordinary gunpowder, in point of effective power, so that it may fairly be said to be a safe and efficient substitute for our old powder.

In support of the above assertions, both of its *inexplosiveness* and *explosiveness*, the author would observe that he has made some trials, which proved conclusively that Neumeyer's powder possessed both those qualities. But as a greater value attaches to trials made publicly, and the results of which have been placed publicly on record, the author prefers to give these in place of his own limited experience of this powder. First, then, as to its inexplosiveness. This was proved by several experiments made in the grounds of the Crystal Palace in December, 1866. The most conclusive test of this quality of the powder was the following: A small house, 5 feet square, built of brick and roofed with slate, and having two chimneys made of 5 in. drain pipes, was constructed, and in it 35 lb. of Neumeyer's powder, half blasting and half gunpowder, were placed. On firing this mass an immense body of flame issued through the openings in the roof, but the powder simply burnt, and moved neither brick nor slate. On 3 lb. only of ordinary gunpowder being placed in the same structure and ignited, a violent explosion took place, which rendered the building a mere wreck.

With regard to its explosiveness, the author has a number of authenticated reports of numerous and varied trials illustrative of this quality. A few are selected which have been made in

mines and quarries in England. The first trials to be noticed were made on the 4th of December, 1866, at the Bardon Hill and the Markfield Granite Quarries, situated near Leicester, and owned by Messrs. Ellis and Everard. The rock at Bardon Hill, which is of a very hard and stubborn character, was rent and cracked in a most satisfactory manner, and a large quantity of material was thrown down, the results being considered highly successful. At the Markfield Quarry one hole was bored horizontally at the foot of an unbroken face of a large extent of solid rock; others were bored vertically. On firing the horizontal hole, the face of the rock was blown out to a considerable extent in every direction, and an unusually large amount of stone was displaced. The vertical shots proved equally successful, and the results generally were highly satisfactory, the quantity of the new powder used being less than that of ordinary powder required for the same amount of work. In a hard, compact rock, too, such as at Bardon Hill, the effect produced by a given quantity of the new powder is much greater than that produced by an equal quantity in a soft or loose rock. It may be as well to mention here that, bulk for bulk, Neumeyer's powder, when well tamped, is equally as strong, if not stronger, than ordinary powder; whilst weight for weight, Neumeyer's powder is the stronger of the two. In point of weight, the new powder is one-sixth lighter than the old, which, supposing we take them at even prices, gives over 15 per cent. advantage to the former, owing to the fact that bulk for bulk (or one-sixth less weight) gives an equal if not a superior result to the best ordinary powder.

Having seen the successful action of the powder upon granite, we will now notice its behaviour in slate quarries. On the 11th of December in the same year five shots were fired at the quarries of the Welsh Slate Company, Rhiwbryfdir, Carnarvonshire. The first shot was in hard rock, the hole being 2 ft. 6 in. deep, and 1¼ in. in diameter; 21 in. of the new powder was used, and was found to do more work than the same bulk of ordinary powder. The second shot was fired in a hole of the same diameter as the last, but 3 in. deeper, cut in the same description of rock; the same depth of powder was used, the result being similar to that obtained with the first shot. Shot No. 3 was in a hole 3 ft. 6 in. deep, by 1¼ in. in diameter, the material bored being pure slate or pillaring rock; the powder filled the hole within one inch, which was occupied by the tamping. The result of this shot was the discovery that the powder wa[illegible] too powerful—a fault certainly on the right side, and [illegible] remedied. The next hole was in the same rock as [illegible]nd was 5 ft. 8 in. deep, with 4 ft. 6 in. of

powder and a light tamping; this gave exceedingly satisfactory results. In another 1¼ in. hole, 4 ft. 6 in. deep, 2 ft. of powder were used, with 2 ft. 6 in. of hard tamping; the result of this shot was decidedly good, the rock being shattered. On the following day three more experiments were made at the same quarries. With 2 ft. 6 in. of powder in a 1¼ in. hole, 3 ft. 6 in. deep, the shot proved much too strong. The second shot was highly satisfactory; but in the third too much power was again developed.

The general result of these experiments is to prove that, bulk for bulk, Neumeyer's powder is much stronger than the powder in ordinary use at these quarries, and which was of the very best description. The question therefore arose as to how the strength was to be reduced when pillaring. It was proposed to have paper cartridges of much smaller diameter than the holes, and which would hold only about one-third or one-fourth of the present charge of powder. These cartridges, it was believed, would answer the purpose exceedingly well in the pillaring rock, where it was desirable to cleave the slate without fracture, and would besides produce a very considerable saving of powder.

A few days after the foregoing experiments, a series of trials were made with the new powder at the slate quarries of Messrs. Matthews and Sons, at Festiniog, Merionethshire. Here two holes 2 ft. deep, in a hard rock of an underground chamber, each half filled with Neumeyer's powder, and two similar holes in a slate rock, were fired with perfectly satisfactory results. Two more shots in the hard rock of the tunnel were not quite so successful; but it was owned that the tamping had been imperfectly rammed, the man having fired them before they were inspected. The two next shots were stated to have done as much with 11 in. of Neumeyer's powder as with 15 in. of ordinary powder. In another hole in very hard rock of the tunnel, the result was completely successful, it being stated that with ordinary powder two holes would have been necessary, or the shot would not have succeeded in effecting the required detachment. A 1¼ in. hole, 8 ft. deep in hard rock in the open air, was charged with 4 ft. 6 in. of powder. This shot was considered very successful, for although not much rock fell, an enormous bulk was loosened, which was readily brought down with a small blast of ordinary powder placed in the rent. Experiments have since been made in various collieries to test the capabilities of this powder in the working of coal, and the results have been exceedingly satisfactory, and have fully borne out the expectations formed. Experiments in the copper mines of Cornwall have also given similar results.

The author could give numerous other examples of trials with

Neumeyer's powder, which fully sustain its reputation as a blasting material; but enough has been said for the present purpose. He has dwelt at some length upon the merits of this material, because it appears to be the only *powder* which has proved in every way superior to gunpowder for blasting, and which really embodies comparative safety coupled with greater energy than the ordinary powder. But although so promising in its character, it has not come into practical use in England. An enterprising gentleman, Mr. E. H. Newby, to whom its introduction in our country is due, devoted a considerable amount of time and money in the endeavour to render it a commercial success. In this, however, he appears to have failed, although it is to be hoped that such an admirable substitute for gunpowder as this material appears to be, may yet find its way into practice. In Germany the author believes it still continues to be extensively manufactured and used.

The most powerful opponents with which gunpowder apparently has to contend, are nitro-glycerine and gun-cotton, and this on account of the extraordinary amount of power they possess; indeed under certain conditions they develop an almost irresistible force. But it is just this attribute of resistless violence which has hitherto rendered them the most unsafe, the most dangerous compounds that can be applied to practical purposes. Man loses all control over these agents, inasmuch as an accidental blow or a slight concussion may—nay, must—produce a violent and perhaps most disastrous explosion. It is of no avail to possess a material which does several times the work of any other adapted for the same purpose, if life and property are in momentary danger of destruction. That this was one of the perilous conditions under which nitro-glycerine and gun-cotton were employed, is evidenced by numerous accidents which have occurred within the last few years in connexion with their application to blasting purposes. But both these dangerous agents have within the last twelve months been brought under control, and their action so modified that they may now be said to possess all the conditions necessary to constitute a safe and highly efficient material for blasting purposes.

Taking them in the order in which they are referred to above, let us first examine the merits and demerits of nitro-glycerine, which is one of the most remarkable materials employed to replace gunpowder as a destructive agent. This substance was discovered by Sobrero, in 1847, and is produced by adding glycerine in successive small quantities to a mixture of one volume of nitric acid of sp. gr. 1·43, and two volumes of sulphuric acid of sp. gr. 1·83. The acid is cooled artificially during the addition of the glycerine, and the mixture is afterwards poured

into water, when an amber-coloured oily fluid separates, which is insoluble in water, and possesses no odour, but has a sweet, pungent flavour, and is very poisonous, a minute quantity placed upon the tongue producing violent headache, which lasts for several hours. The liquid has a specific gravity of 1·6, and solidifies at about 5° C. (40° Fahr.); if flame is applied, nitro-glycerine simply burns; and if placed upon paper or metal, and held over a source of heat, it explodes feebly after a short time, burning with a smoky flame. If paper moistened with it be sharply struck, a somewhat violent detonation is produced.

In 1864, Mr. Alfred Nobel, a Swedish engineer, first attempted the application of nitro-glycerine as an explosive agent. Some experiments were, in the first instance, made with gunpowder, the grains of which had been saturated with nitro-glycerine. This powder burnt much as usual, but with a brighter flame, in open air. When confined in shells or blast-holes, greater effects were, however, produced with it than with ordinary gunpowder; its destructive action is described as having been from three to six times greater than that of powder. The liquid could not be employed as a blasting agent in the ordinary manner, as the application of flame to it from a common fuse would not cause it to explode. But Mr. Nobel has succeeded, by employing a special description of fuse, in applying the liquid alone as a very powerful destructive agent. The charge of nitro-glycerine having been introduced, in a suitable case, into the blast-hole, a fuse, to the extremity of which is attached a small charge of gunpowder, is fixed immediately over the liquid. The concussion produced by the exploding powder, upon ignition of the fuse, effects the explosion of the nitro-glycerine. The destructive action of this material is estimated to be about ten times that of an equal weight of gunpowder, so that if we take 32,832 lb. as the average of work done by 1 lb. of gunpowder, as stated in the early part of this paper, we get 328,320 lb., or about 146½ tons, as the work done by 1 lb. of nitro-glycerine. Therefore, although its cost is about seven times that of blasting powder, its use is attended with great economy, more especially in hard rocks, a considerable saving being effected by its means in the labour of the miners, and in the time occupied in performing a given amount of work, as much fewer and smaller blast-holes are required than when gunpowder is employed. The material appears to have received considerable application in some parts of Germany and in Sweden; but, in England, it has not progressed beyond the stage of experimental trials.

Although nitro-glycerine appears to possess very important advantages over gunpowder as a blasting and destructive agent,

the attempts to introduce it as a substitute for gunpowder have been attended by most disastrous results, ascribable in part to some of its properties and the evident instability of the commercial product. The explosion which occurred on board the West India Company's steamer *European* will long be remembered by many. This distressing event happened on the 3rd of April, 1866, when the *European* was unloading her cargo alongside the railway company's wharf at Aspinwall. The force of the explosion was such as to tear away the upper parts of the ship, and to blow the plates off her sides. The wharf, too, which was some 400 ft. in length, was literally torn to pieces, and about fifty persons killed, whilst many others were seriously injured. By the ship's bills of lading a number of cases of nitro-glycerine were proved to have been on board, and doubtless careless handling of these packages by men who were ignorant of the dangerous nature of their contents led to the catastrophe. As if to impress the public still more strongly with the peril attending even the mere transport of this destructive agent, another accident occurred on the 16th of the same month. Two oil-stained boxes, each measuring about 4 cubic feet, arrived at San Francisco by the Pacific mail steamer. They were removed from the ship into the city, in which they had no sooner been deposited than they exploded with a violence that shook the neighbourhood like an earthquake for a quarter of a mile around, and proved terribly fatal to human life. It was publicly stated that the boxes contained nitro-glycerine, which was intended for sale to the mining companies in Nevada, Idaho, and Collorado. In Sydney, New South Wales, too, a tremendous explosion occurred on the 4th of March, 1866, in the stores of Messrs. Molison and Black, in Bridge-street, which were totally destroyed. The noise of the explosion is said to have very much resembled the discharge of artillery, whilst a column of the *débris* was thrown to a height of about 150 ft. A great amount of damage was done to the surrounding buildings, and property to a serious extent was destroyed. This explosion was traced to two packages of nitro-glycerine.

There is yet another danger attending the substitution of nitro-glycerine for gunpowder in mining, and this relates to its manipulation when being prepared for firing a shot. Although the oil may have been safely transported to its destination, there is no guarantee that its destructive energy will not be developed before it is placed in the hole which is intended for it. Indeed, there r[illegible]nces on record which show how slight a circumstanc[illegible] spread death and destruction around, even in the [illegible]his material. It should be observed that among ot[illegible], nitro-glycerine freezes at a somewhat high

temperature, in which conditiou mere friction will explode it. A sad illustration of this fact occurred in 1867 at Hirschberg, in Silesia, where nitro-glycerine was being used in the boring of a railway tunnel. The oil was one day found to be frozen, and in this state was delicately handled, and fragments were detached by means of a piece of wood. In the bore-holes the frozen nitro-glycerine exploded quite as well as the fluid. One day an overseer attempted to break up a lump of the frozen material with a pick. The result was a violent explosion of the whole mass, which caused the death of the incautious miner. Several accidents have also occurred in our own country since the introduction of nitro-glycerine, and many of those who were the first to experiment with it have already given up its use. This material, therefore, was only worthy of utter condemnation for its fearfully dangerous and uncertain character, even under the most favourable circumstances. Its resistless energy is fully admitted, and its great value in this respect for mining operations duly recognised; but inasmuch as it does not appear that there are any conditions under which it can be handled with safety, its use ought certainly to be everywhere prohibited.

Such being the dangerous character of nitro-glycerine, it is satisfactory to turn to this material in its modified condition. We will now consider this hitherto uncontrollable agent of destruction in a new form, in which it is comparatively harmless, and absolutely far safer to handle and transport than gunpowder. After long and patient experimental research, Mr. Nobel, painfully impressed with the danger attending the use of nitro-glycerine in its ordinary condition, succeeded in effecting a new combination, in which this treacherous compound is rendered perfectly innocuous, except under the actual conditions of work. To this new substance Mr. Nobel has given the very expressive name of "dynamite." It consists of fine gravel saturated with nitro-glycerine, in which condition it presents the appearance of coarse brown sugar. In July last some interesting experiments were carried out with this substance at the Merstham Greystone Limeworks, near Red Hill, Surrey, at which the author was present. So important were these experiments as bearing on the subject of the paper, that the author will here give their details from notes taken by him at the time. The object of the experiments was to illustrate the perfectly safe and harmless character of dynamite under any other conditions except those of actual work, and to show its resistless energy when confined and fired according to the special mode proposed by Mr. Nobel.

A number of cartridges of various sizes were made up of dynamite wrapped in thin paper. To each of them was attached a fuse which burned at the rate of 18 in. per minute.

On the end of the fuse, which was inserted in the cartridge, was fixed a copper cap primed with a powerful detonating compound, and to which is due the development of the explosive energy of the dynamite. A charge of half an ounce of dynamite was first exploded on an oak plank about 6 ft. long, 9 in. wide, and 2 in. thick, and supported at each end. An exceedingly loud and sharp report ensued, and an examination of the plank showed that the charge had taken effect completely through the board, the under side being rent and splintered. A similar charge was then fired on a balk of fir timber placed flat on the ground. A deep indent was made in the timber, and one side was splintered off. To prove the harmlessness of the dynamite when fired by an ordinary light, Mr. Nobel cut a cartridge in two, and lighted one half in his hand with an ordinary fuse. It burned quietly and quickly, but not rapidly, out. The remaining half of the cartridge was then fired with a capped fuse, when a violent detonation resulted. The absence of all danger in case of collision or fire during transport or storage was then demonstrated in a most marked manner. A small deal box, containing about 8 lb. of dynamite, was thrown down from the top of a cliff about 70 ft. high upon a hard bed of rock below. The concussion started the joints of the box, but the contents remained uninjured and unchanged. The test of fire was then applied to a similar box to the last, containing the same quantity of dynamite. A fire was kindled, upon which the box was placed, and after a few minutes the box quietly turned over on one side, a gentle puff of smoke and flame issued from it for a few seconds, and 8 lb. of one of the most violent of modern explosives were almost noiselessly dissolved into air. The charred and blackened box was removed from the embers, and on examination the joints were found to be sound and whole. The author examined this box of dynamite before it was nailed down and placed on the fire, as also the one which was thrown down the precipice, after the occurrence, and therefore writes from his own knowledge of the matter. Such tests ought to satisfy the most sceptical of the safety of the new blasting powder either in a railway collision, or accidental upset of a package, or a fire.

The next point was to test the power of the dynamite when under conditions of partial and also of perfect confinement. To this end about 4 oz. of dynamite were placed upon a block of granite measuring 3 ft. by 2 ft. 9 in. by 2 ft., the dynamite being only covered in with a lump of clay and a shovelful of gravel. A very loud report followed, and on examining the stone it was found to be traversed by rents and fissures, large masses being easily detached by a crowbar. The effect was certainly surprising, considering the comparatively loose and un-

confined condition of the charge. In the next experiment a cylindrical block of wrought iron, about 12½ in. high, and 10½ in. in diameter, and having a 1 in. hole bored through the centre, was used. The bore-hole was filled—but not rammed tightly—with dynamite, and fired. A report soon followed, remarkable for its penetrative loudness, and on examination one-half of the cylinder was found about 80 ft. from the place where it originally stood, being then only stopped by a grass embankment. The other half was found some 50 ft. in an opposite direction, lodged against a pile of broken rock, which stopped its further progress. The iron showed a clean split, which revealed an excellent quality of metal. The bore showed an extraordinary enlargement near the centre, measuring nearly 1¾ in. across, whilst the measurements at the top and bottom of the bore were in each case 1 in., as before firing. It would appear that the power developed increased as it approached the centre of its length, becoming reduced again as it neared the further end of the hole, although of course the explosion was practically instantaneous. Both ends of the bore were open to the atmosphere, there being no plugging nor tamping. The strain on the metal must have been enormous to have thus compressed it around the centre of the bore, and to have rent such a mass and sent its halves yards away in opposite directions.

Although not strictly within the scope of the paper, it may not be uninteresting here to notice the experiments illustrative of the application of this powerful agent of destruction to military purposes. Dynamite is of course unfitted for use either in heavy guns or small arms, its very power being against it in this respect, as forcibly illustrated in the experiment with the cylinder. But it can be utilised in shells with great advantage. A time fuse fitted with the detonating cap would effect its explosion at the proper moment, whilst if the shell broke up in the gun, no harm would result, as demonstrated by previous experiments. The danger attending the use of a shell was too great to allow of its adoption by Mr. Nobel, but he fairly met the point by filling a tin case with 4½ lb. of dynamite, and firing it behind a piece of curved ¾ in. wrought-iron plate, 2 ft. high and 3 ft. long, measured round the curve. The plate was broken into four unequal parts, which were blown considerable distances away. The face of the plate upon which the powder had acted was completely pitted with small holes, due doubtless to the atoms of silica in the dynamite. This experiment satisfactorily demonstrated the great velocity which would be imparted to fragments of shells charged with this explosive.

The next experiment was directly illustrative of the present subject—that of blasting rock. Here a charge of 12 lb. of

dynamite was inserted in a vertical bore-hole 15 ft. deep and 2 in. in diameter tamped with sand. The explosion was indicated by a low subterranean thud and a perceptible tremor of the surrounding land, even at a considerable distance from the blast. The rock showed a series of fissures which indicated that an enormous mass had been loosened, and was ready to be detached by the pick. Had the rock been of a harder and less friable nature, it would have offered a greater amount of resistance, and the whole mass would doubtless have been blown out. This was the case at some granite quarries at Stockholm, where an immense mass was detached by a charge of dynamite, and thrown down in huge blocks. On the present occasion a further charge of 4½ lb. of dynamite was fired at the same depth as the last, with proportionate results. One of our members, Mr. A. E. Walton, who was present at the experiments, and who is constantly engaged in blasting operations, affirmed that the results greatly exceeded his expectations, considering the comparative smallness of the charge. The method of charging in dry ground was next illustrated by filling a glass tube with a series of cartridges which were tamped with loose sand and fired. This experiment was repeated with water tamping to illustrate the mode of operation in wet ground. A striking effect was produced by firing a cartridge in a bucket of water. The detonation appeared to be stronger than under any other conditions; the bucket was shattered, and fragments were picked up several hundred feet from the spot where the charge was fired.

It will thus be seen that the most severe tests for safety failed to show that any danger was present in this material, whilst, on the other hand, there was no condition under which its violence was not developed when fired with a detonating fuse. So far dynamite appears to be well calculated to supersede gunpowder for blasting purposes. The only point of doubt which has arisen in the author's mind is whether any mechanical or chemical change might not occur in the course of time which would render dynamite as dangerous as nitro-glycerine. The author tly made this objection to Mr. Nobel, who, however, stated here was no fear of such an occurrence, inasmuch as he d dynamite in store for very lengthened periods, subject h temperatures, and that it retained its original condition some very trying tests. The stability of dynamite has practically confirmed by extensive and daily use in various , and by the large quantities which are stored at the fac- Beyond this the most careful investigation has shown re slightest ground for apprehension on that ued exposure to the direct rays of the sun last summer, not the slightest chemical

change could be detected, and the same was the case with some dynamite exposed for forty days to a heat varying between 150° and 200° Fahr. All nitrated, or rather hyponitrated organic compounds, are liable to spontaneous decomposition—or what is understood by this hackneyed and ridiculous term—unless they are completely rid of free adhering nitric acid. The reason is that the free acid will produce a local decomposition which sets hyponitric acid free, the latter producing a new local decomposition and so on until sufficient heat is evolved to set fire to the compound. There is no difficulty whatever in ridding dynamite of free acid, but in the case of cotton or any other fibrous substance the utmost care is required, as free acid will sometimes adhere in spite of repeated washing.

In order to bring the matter down to the present time, it may be as well briefly to note the progress dynamite has made since the experiments at Merstham were carried out. In Germany depôts have been established in every mining district throughout the country, and the sale is rapidly spreading. In fact, there are but few mines in the north of Germany which are not more or less using it, and if it has not as yet become as general as gunpowder, it is owing only to the slow-and-sure system peculiar to the German race. The best proof, perhaps, of the great ascendancy of dynamite in Germany is the circumstance that a firm at Cologne, who are the largest manufacturers of blasting powder in Western Prussia, have become converted to dynamite, and have taken up its agency. In Sweden, Mr. Nobel informed the author, that there was some difficulty in prevailing on the miners, who had become used to nitro-glycerine, to use dynamite. But since the prohibition of the liquid explosive in that country in September last, the sale of dynamite has been much larger than that of nitro-glycerine used to be. Sweden consumes at present nearly as much dynamite per month as Great Britain does in a year, which only proves the want of organisation which has hitherto stopped its progress in this country. In Norway the consumption of dynamite is not very large (from about 35,000 to 40,000 lb. a year, the author is informed), but it is steadily increasing. In California dynamite is in great favour, and is transported by rail without any restriction. In the Eastern States of the American Union the miners still continue to use nitro-glycerine, chiefly because dynamite has not been manufactured and sold there.

In England comparatively little dynamite has been used until recently. This is owing to the difficulties of transport, and to the fact that Mr. Nobel has hitherto directed his attention to its manufacture and sale upon the Continent. There is but one depôt for the whole of Great Britain, and that is situated at

Carnarvon. As, however, dynamite is not carried by rail, a great many orders are not executed. Still the quarries of North Wales are regularly supplied, and consume a good quantity. Only three weeks since, a small cargo of ten tons of dynamite in cartridges was shipped from Hamburg to Carnarvon.

The author has referred to several catastrophes which have been caused by nitro-glycerine, but he can only find that a very few have resulted from dynamite. Since the latter material has been introduced, no accident has occurred either from its manufacture, conveyance, or storage. When the nitro-glycerine factory exploded at Stockholm last year, the dynamite stored close by was found scattered about, but not exploded. Two accidents have happened from the use of dynamite in mines. The first was caused by the tamping having been incautiously removed after a miss-fire—an operation which ought not to be allowed in any case. The second was due to the folly of lighting the fuse of a charged cartridge and holding it by the hand until it exploded. These are the only accidents the author can discover. Accidents like these, through carelessness, must and will occur in mines, however safe the explosive may be to handle.

It has been urged in some quarters that the addition of silica to nitro-glycerine constitutes a dead loss of explosive power. This, however, is a mistake. Practically speaking, there is no loss whatever. The dangerous properties of nitro-glycerine, leakage into crevices, and subsequent untimely explosions under the miners' tools, render it absolutely necessary to make use of cartridges, which leave a considerable air room in the powder chamber of the bore-hole. On the other hand, dynamite readily fills up the bore-hole, and leaves no empty space. Practically, therefore, a bore-hole charged with dynamite will hold fully as much nitro-glycerine as when it is charged with the liquid explosive oil, so that the silica is no drawback to its power, whilst it greatly enhances its safety, and renders it far more convenient for use.

The second of the two principal opponents with which gunpowder has to contend is gun-cotton. This proposed substitute was discovered by Schönbein in 1846, since which time it has been greatly improved as far as regards manufacture, and attempts have been made in various countries to apply the material to purposes for which gunpowder hitherto had been alone used. In England, its manufacture on a large scale was commenced by Messrs. Hall, the gunpowder makers, at Faversham. An explosion, however, soon occurred at the works, killing a number of men, the cause being the spontaneous ignition of gun-cotton. This led to the abandonment of the manufacture

on a large scale, and gun-cotton was apparently lost sight of until 1854, when Hadow published some results of his investigations into the nature of gun-cotton. In France, gun-cotton was made the subject of experiment as early as 1846, and its manufacture was carried on at the government powder works at Bouchet, near Paris. Here, however, disastrous explosions also occurred—one in March, 1847, and two in 1848, several lives being lost. These disasters appear to have put an end, until quite recently, to experiments with gun-cotton in France. In Austria, where great attention has been paid to gun-cotton, Baron von Lenk was commissioned to inquire into the merits of the material, and a manufactory of gun-cotton was established at the castle of Hirtenberg, near Vienna. The material was applied for a while to cannon, but soon grew into disfavour, owing to its uncertainty of action. In 1862 an explosion occurred in a magazine at Simmering, near Vienna, which caused the use of gun-cotton in artillery to be put a stop to; and in December, 1866, by order of the emperor, the use of gun-cotton by the Austrian artillery and corps of engineers was entirely prohibited.

Important progress has, however, been made in the development and the practical application of gun-cotton by Professor Abel and others since its study was resumed in this country about six years ago. Very considerable quantities of the material have been manufactured at the works of Messrs. Prentice at Stowmarket, and at the Government Gunpowder Works at Waltham Abbey; its employment as a blasting agent is steadily increasing in several important English mining districts; and some success has attended the employment of gun-cotton cartridges for sporting purposes. The system of manufacture of gun-cotton, as perfected by Baron von Lenk, has undergone but trifling modifications in its employment in this country. It has been made the subject of careful investigation by Professor Abel, and the results furnished by many experimental manufacturing operations, and an examination of the products, have shown that the process of converting cotton into the most explosive form of pyroxilin or gun-cotton, and of purifying the material, have been so greatly perfected by Baron von Lenk as to render a strict adherence to his simple and precise instructions alone necessary to insure the preparation of very uniform products, which are much more pure than those obtained in earlier days.

Although the conclusions arrived at by the many chemists who investigated the composition of gun-cotton, soon after Schönbein's discovery, varied very considerably, the constitution has been very generally regarded as definitely established by the

researches of Hadow, published in 1854. According to that chemist, the most explosive gun-cotton has the composition expressed by the formula $C_6 H_7 N_3 O_{11}$ (which was first assigned to the substance by W. Crum, in 1847), and may be regarded as cellulose, in which three atoms of hydrogen are replaced by three molecules of peroxide of nitrogen. The name *tri-nitro-cellulose* has therefore been assigned to gun-cotton, its constitution being expressed by the formula $C_6 \left\{ \begin{matrix} H_7 \\ 3\,N\,O_2 \end{matrix} \right\} O_5$. Hadow's conclusions have since been confirmed by other chemists, more especially by Redtenbacher, Schrötter, and Schneider, who have analysed specimens of gun-cotton prepared under Von Lenk's directions. An experimental inquiry into the composition of gun-cotton, as obtained by Von Lenk's process, has been instituted by Professor Abel; and the very numerous analytical and synthetical results which he has obtained, confirm the correctness of the formula assigned by Crum and Hadow to the most explosive gun-cotton, and demonstrate satisfactorily that the products obtained by following strictly the instructions given by Von Lenk, are invariably tri-nitro-cellulose, in a condition as nearly approaching purity as a manufacturing operation can be expected to furnish.

Professor Abel has identified himself with the advancement of the gun-cotton question, and great credit is due to him for the light he has thrown upon that question by long and patient experimental research. Still greater credit is due to him for having discovered and perfected a method of treating gun-cotton whereby it is rendered non-explosive when burned in the air, but in which the full energy is developed when fired in a close chamber. The method consists in reducing the gun-cotton fibre to a fine state of division or pulp, as in the process of paper-making, and in converting this pulp into solid masses of any suitable form or density under a pressure of 18 tons to the square inch. This method of manufacture is now carried out by Messrs. Prentice at their works at Stowmarket. To the pulping mainly is due the safety attained, as it insures uniformity in washing, whereby the cotton is thoroughly freed from all acid, and thus every chance of spontaneous combustion is removed. The compression causes combustion to proceed slowly in the open air, owing to the condensed condition of the fibres, which, in the loose state of cotton or rope, burn very rapidly. The gun-cotton is made into small cylindrical charges, which are both portable and convenient for use. The principal of thus combining safety with force in a highly condensed form has produced very valuable results. A series of severe trials, extending over a considerable period, have proved gun-cotton in its present improved form to

possess all the attributes which should be found in a material for blasting purposes. These trials were carried out by Mr. Prentice with the view of satisfying railway companies of the absolute safety of the material. Mr. J. Wilson, the goods manager of the North-Eastern Railway, directed these experiments and made a report thereon, from which document the author obtains the following particulars:

First, a small box containing 125 charges of the gun-cotton, and said to be equal in effect as a blasting agent to a quarter-cask of gunpowder, was ignited by a fuse. When the flame reached the gun-cotton, there was a great blaze like the burning of a heap of loose straw, but no explosion. In less than half a minute there was no flame, except from the burning of the brown paper in which the gun-cotton had been packed inside the box. The box was of wood about ½in. thick, and was nailed, but not bound with iron at the corners; it was one of the ordinary packages used for sending the cotton out. After this a number of charges were laid on the rails near the coal depôts, and coal waggons were run over them, when some of them were ignited, others were not. Some charges were then placed so that an engine should pass over them, and they were all ignited. Mr. Prentice took an axe and chopped one charge into several pieces; there was no explosion or ignition. Small pieces of gun-cotton placed on the iron rim of a wheel and sharply struck with a hammer, exploded, or, rather, detonated. In all the cases where ignition was produced by concussion, whether of a hammer on iron, or of the wheels of an engine or waggon on the rails, it was very evident that only so much as was actually struck exploded or detonated, the part not struck firing from the explosion, and burning like straw or flax. In order to make sure that he was really dealing with the article which produces such an effect when exploded in close confinement, Mr. Wilson had a hole bored in a large block of hard tough wood, in which he placed a charge of gun-cotton with a fuse attached to it. He then filled up the hole with broken slate tightly rammed, and fired the fuse. When the gun-cotton exploded, the block of wood was shivered to pieces, and the fragments were blown several yards away. The result of these experiments convinced Mr. Wilson that railway companies might safely carry gun-cotton along with other goods in ordinary waggons, adopting the same rules as apply to the conveyance of cartridges. The North-Eastern Company, acting on Mr. Wilson's recommendation, now carry the gun-cotton as ordinary goods in covered waggons.

Whilst upon the question of gun-cotton transport, it may be interesting to notice a report of Colonel Boxer upon the com-

parative risks of gun-cotton and gunpowder in conveyance. The colonel is of opinion that no greater risk of an explosion attends the transport of the compressed gun-cotton charges by railway or other modes of conveyance than is incurred by the transport of ordinary gunpowder. He further states that in the event of an accidental explosion of these charges, the amount of damage to life and property would not be greater than that which would result from the explosion of the same bulk of ordinary gunpowder. Finally, he thinks that the mode of packing the charges in deal boxes is preferable to that of packing in strong iron cylinders. The expressions with reference to the comparative safety in accidental explosion are, of course, guarded; but the author feels assured, from comparative trials made by himself on a small scale, that the damage would be much less with cotton than with powder. He has found the compressed gun-cotton perfectly inexplosive so long as the atmosphere has access to it.

The most recent feature in the development of the gun-cotton question is the rendering of the safety gun-cotton, just referred to, violently explosive when in the open air. This is effected by means of a detonating tube attached to a fuse, similarly to the method adopted by Mr. Nobel to explode dynamite. This safety gun-cotton, therefore, which will only burn in the open air if ignited by any ordinary means, will, it appears, develop all its deadly energy if fired under the same unconfined conditions, but with a special detonating fuse. In proof of this some experiments were recently carried out at Stowmarket by Messrs. Prentice. The author appends a few particulars of these trials, which he believes are correct, although he was not present at the time. The first experiment consisted in placing a disc of gun-cotton, weighing about 1 lb. 1 oz., on the stump of a tree lately felled, and igniting it by an ordinary piece of miner's fuse. At the instant of ignition it was enveloped in flame, and moved about for the two or three seconds required for its combustion. About half the quantity was then placed on the same spot, and ignited by a small detonating fuse. A sharp, sudden report was heard, and the stump was found on inspection to be partly penetrated just where the charge had lain, while the twigs of the hedge close by suffered severely. The root of a large tree which lay on the ground was then attacked. A disc of gun-cotton weighing about 1 lb. 1 oz. was placed in a hollow beneath it, a detonating fuse being inserted The explosion shattered the old stump, and scattered its fragments in all directions. The next experiment was calculated to prove the question from a military point of view. A row of palisades composed of trunks of trees, some 18 in. ' eter, and all sunk 4 ft. into the ground, was

provided. A long tree trunk lay touching the foot of the palisade, and upon this 5 lb. of gun-cotton was laid. Wires communicating with a magnetic apparatus were affixed to a detonating tube, which was placed in contact with one of the discs of gun-cotton. Upon the explosion only one trunk was seen to fly away from the spot, and that proved to be the one upon which the charge had been placed; the palisades, although shaken, were comparatively unharmed. A charge of 15 lb. of gun-cotton was then placed against another part of the stockade, which was perfectly sound, and fired. The result was a general smash up, and a tumble over of all the trunks in numerous pieces, and so it ought to have been with such a charge as was employed.

Having brought the history of modern explosive compounds down to the very latest period—for the series of experiments last described only took place on the 22nd of last month—it will be as well, in conclusion, briefly to glance at their comparative merits. Bearing in mind the rule laid down in the opening part of the paper, that a perfect blasting material must be safe to handle and powerful in use, we may dismiss from our minds all those substitutes for gunpowder in which the dangerous chlorates and nitrates occur. The author cannot even make Reveley's white gunpowder an exception, although Mr. Reveley has long made and used it himself with safety. But this is a very different thing to the manufacture, transport, and use by rough hands of hundreds of tons a year. Neumeyer's safety powder appears to embody all that is required as regards safety and strength, although its power falls far short of that possessed by either dynamite or gun-cotton. But as this powder has not been commercially introduced into England, and, in fact, is hardly known beyond the scientific world, it cannot be quoted as a material of which engineers can make choice for blasting purposes. Next in order, we have nitro-glycerine in its improved condition of dynamite. It is between this substance and compressed gun-cotton that the contest appears to lie, and they are both such excellent materials that the probability is they will both succeed in superseding gunpowder in blasting operations. Dynamite has superior difficulties to surmount than those which have attended the development of gun-cotton. The dangerous character of nitro-glycerine has so impressed itself upon the public mind, that it will require time to remove those impressions and to establish confidence in the safe material—dynamite. If we compare the relative safety of dynamite and gun-cotton, there appears to be no difference between them. The only doubt upon the author's mind is whether, after long storage, any

dangerous change may take place in either of the two substances. Mr. Nobel has endeavoured to answer this question with regard to dynamite, but the author thinks a year is scarcely sufficient time to determine this matter in such a comparatively recent discovery. So, too, with regard to the compressed gun-cotton. The author thinks a longer time must elapse before the new form of gun-cotton can be pronounced absolutely safe. The old gun-cotton was supposed to be safe in storage, but accidents at home and abroad have shown the contrary; and, however Professor Abel may now have eliminated the element of danger, as far as chemistry can, it is not for any one to say it is a reliable material until such time-tests have been applied as shall satisfy not only chemical science, but common sense. It is to practical common sense that all these matters ultimately address themselves, and it would be of no avail, commercially, if all the leading chemists in Europe were to pronounce a substance safe if they could not support their opinions by experiments which the public could understand, and in which, therefore, they would place faith. With regard to the question of the relative powers of dynamite and gun-cotton, these appear also to be nearly evenly balanced; we may take it that for all practical purposes they are so. It would appear, however, to the author that dynamite was actually the stronger of the two, for, from his experience of that substance, he thinks that much less than 5 lb. of dynamite would have effected what 5 lb. of gun-cotton failed to do at the palisade experiments—viz., to demolish it. But this is a question that can best be settled by comparative experiments, which the author suggests should be carried out with that view.

It, therefore, results that dynamite and compressed gun-cotton equally fulfil the conditions required of a safe and efficient blasting material as far as science at present can determine, whilst dynamite appears to possess the superior shattering power. It should also be borne in mind that a bore-hole will take up nearly double as much dynamite as compressed gun-cotton, as the dynamite is like moist sand, and can be packed into the irregularities of the hole, whilst gun-cotton is in cylindrical pellets, which can only occupy their own diameter in the hole. Hence, if dynamite was even inferior in point of power, weight for weight, in its application to blasting, it would be preferable. The author does not agree with the theory that the power of gun-cotton is increased by firing it with a percussion fuse. He can only conceive the fuse to act upon the gun-cotton in the way that a whip does upon a horse; it accelerates its action, but does not increase its strength. He thinks that the

same explosive power would be developed in compressed gun-cotton when fired by an ordinary fuse in a confined space, as when fired by a percussion fuse under the same conditions, although it has been demonstrated that the percussion fuse will develop the energy of the gun-cotton in the open air, under which condition the ordinary fuse will certainly not explode, but will only ignite it.

Such, then, is the present practical position of explosive compounds with regard to engineering requirements. The most deadly explosives are at hand ready for work, but as harmless for mischief as so much sawdust or paper. They may be transported with safety, and played with by a child when unconfined; but when imprisoned they will tear down the hardest rock in liberating their gases. These are some of the marvels of the age in which we live, but which some new scientific discovery may eclipse before many years pass over.

DISCUSSION.

Mr. NURSEY exhibited samples of several of the explosive compounds referred to in the paper, and explained their composition, application, results, and other special detail points.

Mr. PRENTICE, of Stowmarket, observed that he regarded the chief interest of the present meeting as having reference to the use of explosives for blasting. He might, however, mention that gun-cotton was used for sporting purposes, and the peculiarity of one of the sporting charges which Mr. Nursey had just laid before the meeting was, that it was perfectly waterproof for all practical purposes. The objection to former preparations of gun-cotton for sporting charges was that damp affected them, and when the gun-cotton was damp, the firing, of course, was not satisfactory. He might, perhaps, call attention to Mr. Nursey's simile between the action of gun-cotton under percussive fire and the whipping of a horse. It appeared to him (Mr. Prentice), that that action was exactly the thing that was necessary, the amount of power being in the gun-cotton, but requiring a peculiar kind of ignition to develop it. The whipping of the horse was exactly the thing wanted. The special method of firing did not add to the power of the gun-cotton, but it enabled the power to develop itself so quickly that the effect was equivalent to an increase of power.

Mr. REVELEY said that he did not regard white gunpowder as a competitor to the special explosive compounds, but simply as an antagonist to gunpowder, which it beat completely both in price and work. He gave examples of effects produced by it,

which it was impossible that ordinary gunpowder could have produced.

Mr. GORE asked whether there was any danger in the use of dynamite after it had been sent to any considerable distance from the place of manufacture. After the explosion of nitro-glycerine at Aspinwall in 1866, a quantity of material similar to what Mr. Nursey had described as dynamite was sent to Mexico, and there it was found that the nitro-glycerine got separated from the particles of silica, and the mixture was quite as dangerous to use as nitro-glycerine.

Captain O'HEA said he had had no experience in the use of gun-cotton, but with reference to gunpowder his experience was that the globular form of grain, as exhibited by Mr. Nursey, was better than the angular form. The former form was sometimes given to Austrian and Swiss powders. The combustion of the globular grain powder was far more even than that of the other kind.

Mr. ALLEN asked whether any experiments had been conducted with dynamite in a frozen condition, as it would probably be dangerous in that state. It would be important to know its behaviour when frozen.

Mr. FEWTRELL said that nitro-glycerine could be rendered perfectly non-explosive for transport by the addition of wood-spirit, the spirit being driven off afterwards by means of heat. He (Mr. Fewtrell), had always looked upon Neumeyer's powder as a weaker form of gunpowder, and he had never before heard so much said in its favour. Several new explosive compounds which had recently been invented had come under his notice. One had been introduced by Dr. Nisser, a Swedish engineer of great experience in mining pursuits. That was really a modification of Ehrhardt's safety powder, and consisted of two compositions, one being little more than chlorate of potash, and the other, sugar, with a little sulphur, and, he believed, some wood fibre or cannel coal. Apart, these substances were perfectly non-explosive, but when mixed together they formed a highly explosive compound. This preparation had been tried on the hardest Cornish and Scotch granites, and he believed the effect had been found to be about six times that of an ordinary blasting powder. Mr. Nursey had condemned the compounds in which chlorate of potash was substituted for nitrate. He (Mr. Fewtrell) thought that those compounds might be made perfectly safe. There was a great prejudice against the idea of using chlorate of potash, but it was perfectly safe if kept separate from the other materials until the powder was required for use. It was only when it was mixed with such

substances as sulphur, or tersulphide of antimony, that it became dangerous.

Mr. NURSEY, in reply, said that he had hoped for a warmer discussion, as he had introduced a variety of subjects, and had expressed some rather strong views with respect to some of them. With regard to the application of the detonating fuse to which Mr. Prentice had alluded, that gentleman would find that his views coincided with those of the author as expressed in the paper. He (Mr. Nursey) gave gun-cotton every credit for being comparatively innocuous in the open air when burnt with an ordinary fuse, at the same time its full violence became developed when fired with a detonating fuse in the air. But the fuse did not increase the absolute power of the gun-cotton, for his opinion was that the same charge fired in a blast hole with an ordinary fuse would exhibit the same amount of power as if fired with a detonating fuse. But in the case of two similar charges fired in the open air with the two different fuses, one would simply burn, whilst the other would violently explode. Mr. Reveley's white powder was undoubtedly very powerful, and he was glad to hear so favourable an opinion upon it and other chlorate powders from Mr. Fewtrell. Mr. Reveley had produced some very remarkable results with that compound, but he had produced them with implements which were not in ordinary use. The bores of both the weapons which Mr. Reveley exhibited were very small, while the metal was very thick indeed, and he (Mr. Nursey) did not believe the same results could be produced with ordinary guns. Mr. Gore had asked whether dynamite had been used in places very distant from the place of manufacture? It had been exported to very long distances, but it had not come into use in California, for the reason that they still had there a very large quantity of nitro-glycerine, and they appeared to prefer the more dangerous material. He had doubts whether there might not be some mechanical as well as chemical change in dynamite which might ultimately render it dangerous. Mr. Allen had implied that there was danger from dynamite when frozen. It was possible there might be danger in the frozen state of the substance, because then the atoms of nitro-glycerine might be in the same condition as they would be in if not mingled with the silica, although he (Mr. Nursey) thought it would hardly prove to be the case, as the atoms of nitro-glycerine were still separated. As regarded the present position of explosive compounds for engineering purposes, he (Mr. Nursey) believed the race to lie between dynamite and gun-cotton. With reference to the spontaneous combustion of gun-cotton, chemists appeared to agree that it was safe in that respect, and no

doubt Mr. Nobel had also satisfactorily proved to his own mind that his dynamite was safe, but he (Mr. Nursey) wished to see the results of storing gun-cotton and dynamite for long periods under conditions of high temperature, &c. He thought that by means of the time test only could the absolute safety of any of the modern explosives be established.

The PRESIDENT said that although the paper was very interesting to the engineering profession, it raised points which lay more within the province of chemists to discuss. Mr. Nursey had exhausted the subject, and the paper supplied a great deal of information. He (the President) had had very little practice with any of the substances mentioned other than the ordinary blasting powder. The members must all feel obliged to Mr. Nursey for the trouble he had taken in producing so excellent a paper.

February 15th, 1869.

F. W. BRYANT, PRESIDENT, IN THE CHAIR.

THE DRAINAGE AND WATER SUPPLY TO THE TOWN OF FAREHAM.

BY THOMAS BUCKHAM.

THE town of Fareham, in the county of Southampton, contains 900 houses, and, by the last census, 5000 inhabitants, and is situated at the head of the north-western estuary of Portsmouth Harbour. Previous to 1860, the greater part of the houses were drained into cesspools, very imperfectly constructed, and in close contiguity to the wells; in all cases the soil stood higher than the surface of the water. The strata being gravel, the liquid contents of the cesspool permeated into the wells, poisoned the water, and consequently produced many kinds of dangerous diseases. Those houses which were not drained into cesspools, discharged their sewage into open ditches, and created pools of stagnant water, thereby generating putrefaction, and in many instances in close proximity to the dwellings of the poorer classes. A well and the large cesspools of two closets in the principal street of the town existed within six feet of each other for years previously to its being drained. The water of this well was used by the family for ordinary domestic purposes. Many similar cases to this in the thickly populated part of the town were found by the author in the course of the drainage being carried into effect. Some houses and cottages in Quay-street were in a very unhealthy condition before the present system of drainage was carried out. The surface of the yards was unpaved and in a most filthy state. There were pigsties and closets abutting on the cottages. The ground was saturated with water almost to the surface. The refuse in the ditch was of the most filthy and abominable kind, and stood stagnant within a few inches of the level of the adjoining cottage floors, and in actual contact with some of the cottage walls.

At an inquiry held at Fareham in 1849, before Robert Rawlinson, Esq., C.E., as to the sewerage, drainage, and water

supply, and the sanitary condition of the town, Mr. John Blatherwick, surgeon, and many other old residents, stated in evidence that the place was totally without drainage, neither was there any means of a good supply of water; that a great amount of disease existed in the town at that time, and many other details which proved, in their opinion, that the only effectual remedy was a thorough system of drainage and water supply. Mr. William Pink stated: "My well is situated about 9 ft. from the cesspool, and is about 20 ft. deep. Our water was in such a state four years ago that we could not drink it. We opened the well, and then discovered streams of putrid matter from the cesspool. I went to considerable expense in puddling up this to cut off the soak. It cost about 4*l.* It is not effectual, I would rather buy water than drink our own. When we opened the site, the gravel was black. I have no power to make a drain, or I would gladly do so. We are annoyed during wet weather with springs which ooze up through the floor."

The whole of the public and private works have been carried out under the direction of the Fareham Local Board of Health, in conformity with the provisions of the Public Health Act, Local Government Act, and the Acts incorporated therewith. The author, who was surveyor to the Local Board, received that appointment in September, 1860, when portions of the sewers and water mains were laid. Since that date many alterations and additions have been made in the general system, from the designs and under the direction of the author. A system of pipe drainage from, and water supply to, every house in connexion with the sewers and water mains has been constructed. Every common privy has been converted into a water-closet, and every cesspool filled up and destroyed. The drainage from houses to the sewer consists of glazed earthenware socket pipes jointed in cement, with an average fall of 1 ft. in 35 ft. The pipes are all of 6 in. diameter, with 4 in. branches to sinks and surface gratings. The water-closets are fitted with glazed earthenware pans, and P traps built into brickwork and set in cement. The quality of the pans differs, but the majority of closets (those to cottages) are fitted with neat ones at a cost of 7s. 6d. each, which have been found to answer every purpose.

Immediately on the completion of the main lines of sewers and water mains, the Local Board put in force their powers under the Acts of Parliament, and served a notice upon every owner of property in the town to lay on a system of drainage and water supply, and to employ such persons to execute the work as they should approve of. The drainage of the houses which were owned by persons of limited means was executed by the board and a rate called a "Private Improvement Rate"

made on the property, which extended over a period of thirty years. In Gloucester, Hereford, and many other towns where a system of drainage has been completed many years, the branch drains appear to be quite neglected, whereas they are of much more importance than the main lines. Previously to the appointment of the author as surveyor the Local Board approved of a scheme, and had decided to erect a pumping station at a point where the London and South Western Railway crosses Fareham Lake, for raising the sewage of the lower district into the sewers of the higher one, for the purpose of sending it with that of the whole district to the main outlet. The board were also under the impression that this was the only means of disposing of the sewage of this portion of the town, if the drains were laid at a sufficient depth to drain the cellars of the whole of the houses. The author, upon a survey of the district and after taking the necessary sections, decided that a separate outlet could be obtained, and that the pumping establishment (which would have been a perpetual expense) could be dispensed with. He therefore prepared plans and sections for the drainage of this portion of the town, which were adopted by the board, and the works were carried out. The system comprises a small set of deodorising tanks, which are constructed at the extreme end of the north-western estuary of Portsmouth Harbour. The outlet is a 12 in. cast-iron pipe, jointed in lead, and supported by piles; 12 in. cast-iron pipes are also laid from the inlet end of the tank across the mill-pond, and are connected with the pipe sewers in the centre of the road. The 12 in. pipe sewer from this point is constructed with a gradient of only one foot in 1300 ft.; it has now been in use nearly seven years, and not the slightest obstruction has occurred. The sewers are of ample depth to drain the whole of the houses in that neighbourhood. The difference in the cost between the pumping station and the scheme designed by the author, was 2500*l.*

The water services to houses consist of $\frac{3}{4}$ in. galvanised iron high-pressure pipes, connected to the top of the water mains by $\frac{3}{4}$ in. circular brass ferrules; a stop-tap is placed in the most convenient point nearest the water main so as to enable the water to be shut off in case of leakage, or repairs, &c.; $\frac{3}{4}$ in. high-pressure bib-taps are fixed in convenient places for the supply of water for domestic purposes. The water is laid on to every water-closet, and a self-acting valve fixed for the purpose of supplying the pan with water when required. The system of connecting the water service direct to the pan of the water-closet was adopted by the Local Board for the purpose of reducing the expense to the owner as much as possible. The author strongly objects to this system if the waste of water is of

any material consequence. He has found very many cases, during the last two years of his residence at Fareham, where the handles of the valves have been propped up for two or three hours at a time, the water continually running to waste, thereby involving a very considerable loss. This extravagant waste of water can be prevented by the adoption of waste preventive systems fixed in some convenient part of the closet over the pan; one which the author has observed used very extensively and effectually in Southampton, can be procured at a cost of 21s.

The drainage to the town consists of glazed earthenware socket pipes, of sizes varying from 9 in. to 18 in., and is divided into two districts, with a separate set of deodorising tanks for each. The entire length of the sewers is four miles and seventy-six chains. The whole of the surface water of the town is drained into the sewers, the necessary gullies being constructed at various points for that purpose. The author, not finding the sewers sufficiently large to take water during a storm, designed and constructed two 12 in. storm outlets, the bottom of the 12 in. pipes of the outlets being placed level with the top of the sewer, so that immediately the sewer becomes full, they are relieved. The tidal valves are self-acting, and consist of a 15 in. copper ball which rises with the tide into an india-rubber ring bolted between two iron plates, and effectually prevents any water gaining access to the sewer during high water. The details of the outlets are shown in Figs. 1 and 2. On several

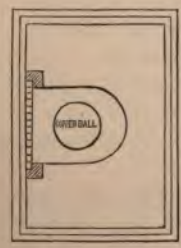


FIG. 1.

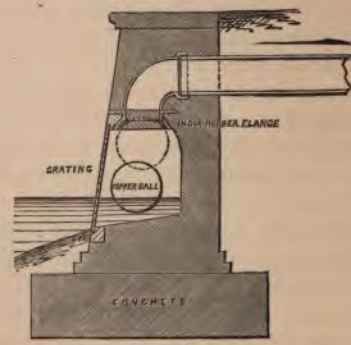


FIG. 2.

occasions before the construction of these storm outlets during heavy rains the sewers became full, and flowed into the cellars of many of the houses. Since these additions there has not been a repetition of that defect.

Ventilating shafts are constructed in the highest parts of the town, and at the ends of the sewers. Cast-iron boxes filled with charcoal a for the purposes of ventilation—the charcoal requir ing every month. Flushing shafts are

also constructed at the ends of the flat sewers, and are used in the dry summer months about once a week. The water is laid on to the shafts from the water mains by 3 in. branches, with a sluice fixed to it for the purpose of turning the water off and on. A flushing valve of galvanised iron is fixed in the end of the pipe, and when the shaft is full it is raised with the aid of an iron chain attached to an iron box with lid, &c. Examination shafts, shown at Figs. 3 and 4, are also placed in convenient

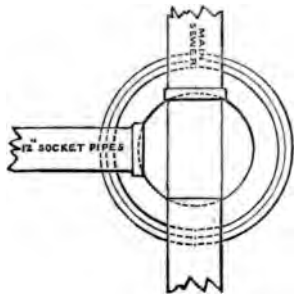


FIG. 3.

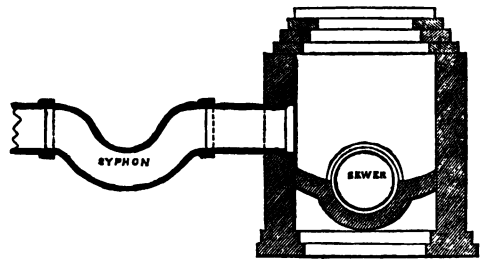
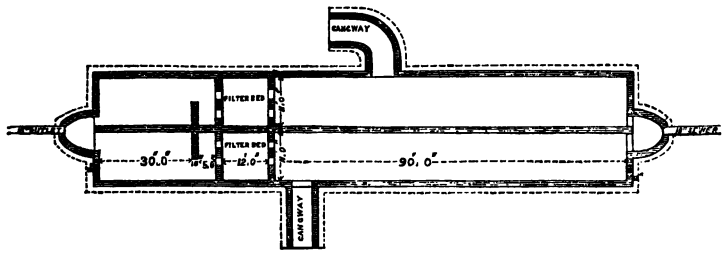


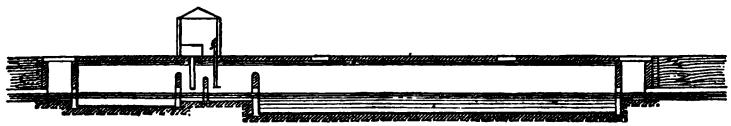
FIG. 4.

places on the sewers; they have cast-iron doors and frames fixed 3 in. below the surface of the road.

Figure 5 shows a plan and section of the tanks used for the



Plan.



Longitudinal Section.

Transverse Section.

FIG. 5.

purpose of deodorising the liquid portion of the sewage, previous to its discharge into Fareham Lake. It consists of two sets of tanks; the one for use during the time the other is being cleansed. The chambers for the retention of the solid portion of the sewage are 90 ft. in length by 11 ft. in width, and the floor is 4 ft. 6 in. below the level of the inlet and the bottom of the filtering bed, which is filled with rough gravel for the purpose of filtration. The chambers at the outlet end are for the purpose of retaining any solid matter which may find its way there, to prevent its discharge into the lake. Two 18 in. iron sluices are placed at each end of the tanks to divert the sewage when required. A building, 20 ft. by 10 ft., is erected over the filtering chamber, and contains a large galvanised iron tank which is used for mixing the lime with the water. The whole of the brickwork, with the exception of the arches, is built in cement. The cost of the tanks and outlet was 1050*l.*, and the working expenses, including lime, 70*l.* to 90*l.* per annum.

The solid portion of the sewage is retained in the two large chambers, and when filled is cleaned out and sold for agricultural purposes at 3s. per ton; the quantity of solid manure collected in the tanks is between 400 and 500 tons per annum. The sewage is purchased by farmers in the neighbourhood, and applied to the land in different forms; some preferring it mixed with ashes, and others as it is taken from the tanks. The retention and sale of the sewage is profitable, but the farmers having but little experience of its utilising qualities do not think it at present of more value than 3s. per ton. The deodorant used is milk of lime.

The deodorising of the liquid portion of the sewage previous to its discharge into the lake is not effectual, and the fact of its being discharged imperfectly purified has been a serious annoyance to the immediate residents of that neighbourhood. The author is of opinion that to deodorise sewage in a very moderate way, a constant stream of lime and water, or other deodorants, should be constantly running into it before it enters the stream. This was not done in this case, but the tank was filled once a day only, and the contents discharged into the chambers before alluded to; the consequence was that the sewage was washed up and down the lake by the ebb and flow of the tide, and necessarily became embedded on the banks which were exposed at low water, thereby causing the annoyance before mentioned. The difficulty of disposing of the sewage of towns so as not to pollute the rivers and streams in the vicinity is a very great obstacle to sanitary improvements, inasmuch as in obviating one evil another is created. In the adoption of the system of irrigation in many towns, nothing has been done which it can be said actually meets the necessary requirements.

The pipes from the tanks to the outfall are 15 in. cast iron, jointed in lead. The tidal valve at outfall is constructed of brickwork in cement, and has a beech apron which closes by the pressure of the tide. The whole of the pipes used in the public and private drainage of the town have been properly laid. The sewers have been used now nearly seven years, and not the least failure or stoppage of any kind has occurred. Any failure in pipe drains with an ordinary fall is the result of some accountable cause, such as the joints being imperfectly made, so that the cement finds its way into the inside, where it forms a hard ridge which would obstruct the flow of the sewage.

During the time the author had the honour of discharging the office of surveyor to the Fareham Local Board, he personally superintended the laying of the pipes used in the drainage, and several times found an attempt by the workmen to increase or diminish the size of the drains by pushing the end of the smaller pipe into the larger one, also forming a junction of one drain with another by cutting a hole in a pipe in which to insert another; the ultimate result of this practice is of course a failure to the scheme. Efficient professional supervision is essential to the proper formation of sewers and drains, and if confided to bricklayers, or persons having no professional experience, the consequence is, increased expenditure and certain failure.

The water supply consists of a covered reservoir shown in plan and section at Fig. 6, of sufficient size to hold 100,000

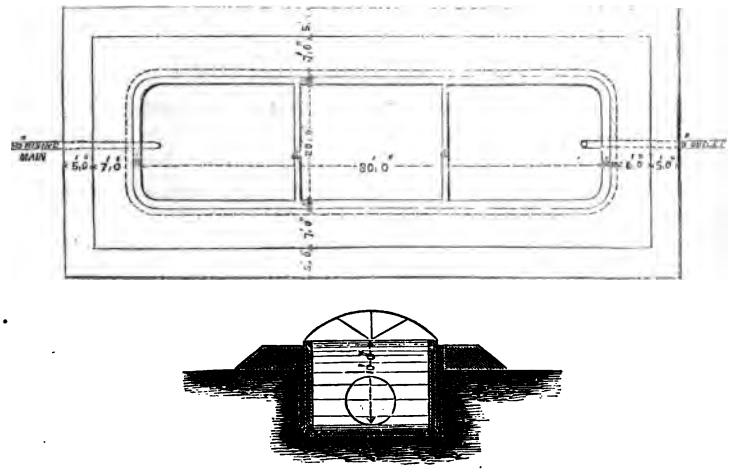


FIG. 6.

gallons, the level of which is about 100 ft. above the lowest part

of the town, and 50 ft. above the highest. The mains vary in size from 2 in. to 8 in., and the entire length is 4 miles and 60 chains. The supply of water is obtained from two wells sunk in an old chalk pit (called Main Dell), situate at the western end of Portsdown Hill; the bottoms of the wells being about 45 ft. below Trinity high-water mark. A large heading, 40 ft. long and 20 ft. in height, connects the two wells. The water is raised into the reservoir by three 10 in. pumps, worked by two 14 horse power beam engines, constructed upon the compound principle. The pumps are capable of lifting 15,000 gallons of water per hour 110 ft. in height. The consumption of coal by the two Cornish boilers is 2½ lbs. to 3 lbs. per horse power per hour. The supply from the reservoir into the town is constant, day and night, and in order to preserve a uniformity of quantity throughout the service, all the mains communicate with each other, so that a free circulation is kept up, by which means, from whatever quarter the draught is made, the water will flow from all other quarters towards it. Sluices are placed at convenient places throughout the mains for the purpose of shutting off the water for repairs and regulating the pressure. Hydrants are fixed in the footways at distances of about 80 yards apart. The whole of the mains are laid in a separate trench from the sewers, this plan being adopted to insure greater safety; the depth of the mains below the surface of the road being 2 ft.

The quantity of water used in the town in the 24 hours, on an average, throughout the year, for domestic purposes only, is 240,000 gallons; being at the rate of 48 gallons per head, and 240 gallons for each house throughout the town. This very extravagant waste of water is caused solely from the very fact of the water closets having a direct connexion with the mains, and the handles of the closets being propped up in the manner before described. The author found, on a visit to the town a few weeks since, that in consequence of the continued great waste of water, the Local Board had resolved to cut off the supply during the night. This would be a very serious matter in case of fire, as it would involve a very great delay by the time occupied in giving notice to the turncock to open the sluice valves.

The author's opinion is that to carry out a thorough system of town drainage (particularly pipes), it is essentially necessary to combine with it a good supply of water for flushing purposes, and that, with it, sewers of much smaller dimensions would be as efficient as larger ones without it.

The annual expenses of working the engines and pumps—including coals, repairs, stores, and engineman's wages, is 170*l.* Four rates of 9d. in the pound covered the whole of the expenses for the twel[illegible]ths. Mr. William Bellingham, of Fareham,

was the contractor for the construction of the main lines of sewers and water mains. Mr. James Tayler, of Fareham, for the house drainage and water supply; and Mr. Thomas Horn, of Millbank, Westminster, for the erection of engines and pumps.

DISCUSSION.

Mr. LATHAM asked the author what portion of the scheme he considered himself responsible for.

Mr. BUCKHAM said that a third of the scheme had been carried out by Mr. Routledge, when he was appointed surveyor to the board, but that he (Mr. Buckham) held himself responsible for two-thirds of it.

Mr. LATHAM considered that, looking at the requirements of the town, the plan as laid down was extremely crude. He did not see a single man-hole or inspection hole, and all the lines of sewers were laid out in curves, which was contrary to the principle now adopted by engineers for town drainage. The town it appeared had two outlets, one upon the high and the other upon the low level, but for all practical purposes, the sewers were found insufficient to carry away the ordinary storm waters, and certain storm outlets had been designed in order to relieve the sewers. He would ask the author whether those storm outlets had any effect upon the low level system of sewers, for he could readily conceive that, in the case of sewers of such low capacity, if a storm occurred, the same evil results would still follow as had hitherto happened in the town, and that no storm outlets of any description would be a remedy. The author stated that, "in the adoption of the system of irrigation in many towns, nothing has been done which can be said actually meets the necessary requirements." Now, what were those necessary requirements? Perfect purification of the sewage, and prevention of the pollution of all our rivers, and how best had these requirements been met? Had the end been obtained by the lime process? Take any town, from the largest to the smallest, which had adopted the lime process, and the result had been a failure. That was to be seen everywhere, and proceedings in the Court of Chancery were constantly showing that where the lime process had been adopted it had proved to be a signal failure. The process had also been shown not only to be a failure, but the result of the application of lime, let it be applied as perfectly as possible, was, that it was very injurious as regarded fertilising power. When he (Mr. Latham) knew that the system advocated in the paper had failed, he could not, of course, support it, especially as the author had followed up his description by observations derogatory to that natural system of

treatment—the irrigation system—the principles of which were correct. The irrigation system was already becoming the national system, for a recent Act of Parliament relating to the drainage of towns had also made provision for the acquisition, on the part of towns, of lands whereon they could utilise that element of fertility which, if utilised rightly and properly, would be of immense value, and the utilisation of which would effectually prevent the pollution of our streams. With regard to the ventilation of the Fareham sewers, the author stated that iron boxes containing charcoal were used. He should like to know how many were adopted, and in what position they were used. The ventilation of sewers had been a vexed question with many drainage engineers, and that was because sewers had been constructed without any provision for ventilation. In providing for ventilation, the system should be laid out so that every part should be under command for the purposes of ventilation. It was of no use to put a box at the top of one sewer, and call that a ventilator for the whole system, as it would tend to take the miasmas which had been produced in one quarter, and convey them into the highest and best parts of the town. A perfect system of drainage would be laid out with a view to ventilation, and it ought to be so broken up, that it would be impossible for gases to pass far along the sewers without escaping. It seemed from the paper that no regard had been paid to that at Fareham, and that the whole of the gases were allowed to sweep through the town, and escape by a few outlets at the head. That feature would sooner or later be found to be a weak point in the scheme. For a few years the Local Board might find the town all that they required, and believe that they had carried out their sanitary works perfectly, and yet at last be visited by some particular type of low fever for which they could not account. It was well known that whenever a certain volume of water or sewage was discharged into a drain it would displace an equal bulk of mephitic air. And the house being the weakest point, partly on account of its high temperature, the foul air would be most likely to enter it. Rain-water pipes could not effectually act as ventilators if they were not constructed for that purpose, for at the time of storms when they were required to do duty as ventilators, they were acting in their proper capacity of rain-water pipes.

Mr. Church said that he fully indorsed the remarks of Mr. Latham, as to the necessity of ventilation, and he was of opinion that where a proper system of sewage had failed the failure had been entirely due to defective ventilation. He did not wish t[illegible] disparagingly of the paper, because any one who brou[illegible]er before the Society was deserving of credit.

But no one ought to allow to pass unnoticed a principle which in his opinion was incorrect. The Fareham sewerage appeared to have several radical errors in it. Modern engineers must acknowledge that the method of disposal there adopted was incorrect. He believed that the question of the utilisation of sewage was fully settled. He did not know any case in which the ulitisation system, properly applied under skilled engineering superintendence, had been a failure. In the town of Chelmsford, with the Local Board of which he was connected, they had had some considerable difficulty in the disposal of the sewage. The sewers were carried out about fifteen years ago, and at that time the general notion was to put the outfalls into the rivers. The outfall in the case of Chelmsford was put into the river Chelmer, and the sewage became a source of pestilence to the inhabitants, and blocked up all the mills. The Vice-Chancellor was appealed to; an injunction was granted to restrain the local authorities from putting the sewage into the river; the whole question was considered, and the sewage was now being utilised most successfully; although the land had a very wet bottom, the yield of Italian rye-grass had been from forty to fifty tons per acre. Another point to which he would allude was that the paper mentioned a pipe sewer with a gradient of one in 1300 ft. Such a gradient was altogether inconsistent, and one which was practically ineffectual.

Mr. Olrick said the paper ought to have given an analysis of the water supplied to the inhabitants in order to show its quality, more paticularly as it was laid on direct from the mains into the houses, and there were no intermediate cisterns. Cisterns were failures in a great many cases because the favourite place for the cistern was at the top of the water-closet. In many cases the water-closet would be out of order, and, consequently, the stench from the water-closet would get absorbed by the water. In such cases, by the time that the water was about to be drunk, it had become deteriorated by a great amount of decomposed organic matter. In his own house he used an animal charcoal cistern-filter, and he found that it was the best remedy against drinking impure water, because it filtered the water just at the time it was required for use. In the case of Fareham the water was supplied direct from the mains, and there was no possibility of filtering it at the time of drinking it.

Mr. Usill said that whatever the demerits of the Fareham drainage might be, he did not consider that Mr. Buckham was responsible for them, as he took office when a third of the works had been carried out, and he had to complete the plans commenced by his predecessor, and he made such improvements as he deemed necessary, and the state of the works admitted.

He (Mr. Usill) could not agree with the remarks of the author that as yet nothing definite had been settled as to the merits of the system of irrigation. The successful results which had been obtained at Croydon and elsewhere, gave sufficiently conclusive proof of the superiority of irrigation over all other schemes.

Mr. FISHER asked what data were given for the thickness of the stoneware pipes, and how the pipes were jointed. The paper stated that the house drainage was jointed with cement, but it stated nothing about the other drains. If the drains were jointed with cement how was a stoppage got at? He also wished to know what sort of ventilators were used for holding the charcoal.

Mr. BUCKHAM, in reply, said that his object in bringing the drainage of Fareham before the notice of the Society was to raise a discussion. The man-holes were placed at the tops of sewers with steep gradients, and very near together in sewers with flat gradients, so that there was an ample opportunity of getting at the sewers and giving them a good flushing without affecting the pipes. The storm outlets were designed by him, and, on a recent visit to Fareham, after an absence of five years, he found that they were acting most efficiently. Mr. Latham had remarked upon the disposal of sewage, but he (Mr. Buckham) questioned whether Mr. Latham's experience was sufficient to enable him to form an adequate opinion as to the results of irrigation. He (the author) acknowledged that the practice of discharging the sewage from the town of Fareham into the estuary of Portsmouth Harbour was not effectual, and the reason was, because it was not properly deodorised. There was much to be said in favour of the adoption of sewage for the purpose of fertilising land. There was no doubt that it was the most efficient way of using it. The statistics from Barking, which was said to be the best irrigated farm in the country, showed that the crops averaged 60 tons per acre, and that last year the yield was 70 tons. Mr. Latham had stated that lime did not destroy the fertilising qualities of the sewage. He (Mr. Buckham) was afraid Mr. Latham had never tested that point. In the district of Fareham, where the lime had been mixed with the solid portion of the sewage, the farmers stated positively that it destroyed the fertilising property of the sewage, and he had visited several farms where the lime and sewage had been placed upon the land, and found that they were inferior in condition to their state before they were so treated. They were afterwards treated with sewage in which there was no lime, and the result was much better than when the lime was used. With regard to the question of ventilation in the Fareham drainage,

he could assure the meeting that every attention had been paid to that point. As to the flow of sewage through drains, no two engineers in the metropolis would agree on that point, so that it would have been useless for him to give data as to the flow, for nobody would have believed it. The actual flow was 240 gallons per house per day. Mr. Church said that a gradient of 1 in 1300 in a sewer was a mistake; but his (Mr. Buckham's) experience was, that there was not a sewer in the Fareham district which acted better than the one which was laid with that gradient. In the metropolis, sewers had been laid with a fall of two feet to the mile. He had recently constructed thirty-two miles of sewers for the parish of Battersea. One of those sewers had only a gradient of 1 in 2600, and it acted very well. That, however, was nothing to what the Metropolitan Board had done. The Southern Low Level was constructed with a fall of two feet to the mile. Where there was a supply of water for flushing, a sewer with a gradient of 1 in 1300 was a very good one. Referring to the water supply, he would observe that the advantage of having the mains attached to the houses was very great. A constant supply could be got without the labour of pumping, and in case of fire there was the water ready to extinguish it. A filter bed was not required, because the water came into the well at the rate of 15,000 gallons per hour. It was thence pumped into a reservoir, and distributed amongst the inhabitants. The Fareham reservoir was constructed with perpendicular walls, 18 in. in thickness, puddled at the back with puddling 12 in. thick. There were cross walls, as shown on the plan, to give support to the others, and those walls had circular holes to allow the water to pass from one part to another. The pipes at Fareham were jointed in cement, and every tenth pipe was a half-socket pipe, so that it could be easily removed in the case of any stoppage on the insertion of a junction. He (Mr. Buckham) was happy to say that during the eight years the Fareham sewers had been in action, no stoppage had occurred. The depth of the sewers throughout the town varied from 4 ft. to 9 ft. He believed that the Fareham system of sewerage was perfect as regarded drainage and water supply, for every house in the town was connected with the sewers, and the water was laid on at all the houses for the closets and domestic purposes. In reply to a question as to the former and present death-rates in Fareham, Mr. Buckham stated that in 1858, it was 16 per cent., and now it was more.

Mr. Church said that he considered that the question of the rate of mortality was most pertinent, because Mr. Buckham had spoken of the system as being most perfect. He had, however, now almost in the same breath shown it to be most imperfect.

With regard to gradients, he must protest against its going forth from the Society that two feet in the mile was sufficient fall for a pipe-drain. It might be, and no doubt was, sufficient for the metropolitan drainage, where the sewers were 7 ft. high, but they were not to be compared with drains of 18 in. in diameter. He protested, as an engineer, that the gradient, to which he had taken exception, was insufficient.

Mr. BUCKHAM said that the reason he had stated that Fareham was the most perfectly drained town in England was, that every house was connected with the sewers, and that every house was connected with the mains for a supply of water. The death-rate might have increased from the fact that the sewage flowed into Portsmouth Harbour, which ebbed and flowed, and of course, gave off noxious gases.

The PRESIDENT, in closing the discussion, remarked that although the paper had elicited an expression of differing opinions with regard to the scheme of drainage it described, they were indebted to Mr. Buckham for bringing the subject before the Society. The question of sewage was one of the utmost importance in the present day. The general feeling of the meeting seemed to be decidedly in favour of irrigation, in opposition to the plan which Mr. Buckham had been carrying out at Fareham. The author of the paper had remarked that sufficient time had not elapsed for Mr. Latham to be able to state with certainty whether irrigation would be permanently successful, but still Mr. Latham had the advantage of seeing that, wherever his system of irrigation had been carried out, the death-rate had decidedly decreased. Mr. Buckham's scheme, however, was not in the same favourable position on that point. He, the president, congratulated the Society upon the interesting information which had been elicited during the discussion.

March 1st, 1869.

F. W. BRYANT, PRESIDENT, IN THE CHAIR.

ELECTRIC TELEGRAPHY, IRRESPECTIVE OF TELEGRAPHIC APPARATUS.

BY E. G. BARTHOLOMEW.

THE extent of this interesting field is such that it is not attempted within the limits of a paper to enter fully into every point of detail which the subject embraces, and the author has, therefore, deemed it sufficient for the purpose to select such features as either in his estimation present difficulties to the less initiated, or contain within them grounds for discussion. He is aware that many facts connected with every subject, which, to those whose every-day life is more or less associated with it become matters of course, are nevertheless to many replete with that which requires explanation, but as it is hoped such points will crop up in the course of the discussion, he is content to confine himself for the present to what appears to him worthy of notice. It may be further added that matters which possess interest, and nothing more, have been avoided; and, moreover, that as it would have been impossible to do justice to the subject of telegraphic instruments and apparatus upon the present occasion, any allusion to them in respect either of their construction or use has been omitted. The allusion to a galvanometer in connexion with inductive charge and discharge has been necessary for elucidation.

The subject may be divided into the two branches which appear to be both obvious and necessary, since each branch contains facts totally distinctive and peculiar to itself; these branches are: telegraphy as applied to land lines, and telegraphy as applied to submarine lines. There is also a third division of the subject—subterranean telegraphy—which occu-

pies, as it were, a middle position between these two, and which is better considered in connexion with submerged lines.

The development of land telegraphy had, until within the last four years, attained a decidedly higher stage than had submarine telegraphy. The experience gained in the one had been greater —the subject did not in itself present so many difficulties—and it did not involve a knowledge of certain physical facts which it was evident, even in its earlier days, the latter necessitated, and which became more obvious as the subject progressed. It is mechanical knowledge which assists us the most in the construction of a land line, whereas the manufacture and submergence of a telegraphic cable involves, besides that, in a much higher degree, an acquaintance with many chemical facts, and a knowledge of the laws by which electricity is governed, to a much greater extent than is necessitated in the former case.

First, with respect to land lines: the conductor demands special attention. If a completed line of telegraph be compared to a "permanent way," the conductor or line wire represents the "metals." It is usual, or at least desirable, to proportion the weight of the rail per yard to the traffic expected, and it is equally necessary that the conductor in a line of telegraph should be proportioned to the work it has to perform. It must not, however, be exactly proportioned, as will be seen from a further consideration of the subject. The duty of the conductor is to convey the electric current at all times when required, and under all circumstances which may arise, without opposing unnecessary resistance to its progress. A certain amount of resistance is unavoidable, but when this becomes excessive it will produce a variation in the carrying capacity of the wire under differing atmospheric conditions, besides causing uniformly indistinct signals, unless the undesirable alternative be adopted of increasing the battery power in order to compensate for, or overcome, the resistance.

The general question of resistance (R) is one of serious moment, and of great interest in all electro-telegraphic matters, and although this remark more fully applies to submerged conductors, yet it must by no means be lost sight of in open wires. It is dependent upon three conditions: the length of the conductor, its sectional area or diameter, if round, and the material it consists of. All substances are conductors of electricity to a certain extent, and all substances resist its passage to a certain extent. The two characteristics are the converse of each other; those which conduct the best resist the least, and *vice versâ*. That a worse conductor m[illegible] made equal to a better by increasing its sectional ar[illegible] its weight per mile. Thus, the conductivity of pure c[illegible] bout eight times that of iron,

but an iron wire of ·3 inch will equal in conductivity a copper wire of ·1 inch, from which it appears that R is in direct proportion to the area of the cross section. Again, R being in inverse proportion to the length of the conductor, a line of one mile of iron wire will equal in R a line of eight miles of copper wire, their diameters being the same.

There are several reasons why, in suspended lines of telegraphy, galvanised iron wire has become the universally adopted material for the conductor. It is less expensive than copper—it will bear a higher strain without breaking—and it is not subject to rapid deterioration. There are two tests which the wire should undergo before being suspended—that is, if it is a solid and not a stranded wire which is to be employed; a short sample from each delivery should be held firmly by one extremity, and a narrow streak of ink or some black pigment should be drawn down it upon one side from end to end in a straight line. The free end being then attached to a cross handle, the wire should be twisted in the same direction until it breaks; the number of twists per foot can then be counted by the black spiral formed. A good No. 8 wire will stand from twenty to forty twists per foot. But the most necessary test to which a wire can be subjected is that of longitudinal strain. This must not be confined to short lengths. The quality of the iron may be good, but from want of care in making the welds there may be faulty places, and if the sample subjected to this test happens not to contain a weld, it may pass muster. The straining of a sample will prove the quality, but in the erection of a telegraph this is not sufficient. In this case, the whole length of wire, after being drummed out, should be tested in sections of from a quarter of a mile upwards, according to circumstances. The strain should be applied until the wire ceases freely to elongate, long before which period all bad joints and faulty places will have given way, and having stood this strain, the wire will be found to have lost all its "spring," and to be more convenient for handling. In this state it is said to be "killed."

With respect to the gauge of the wire, it has been shown to depend upon the length of the circuit of which it forms part. No doubt, in view of the law of R, every different length of circuit might have a different sized conductor, but other considerations step in. Suppose a wire to be just sufficient in its capacity for its work when newly erected, it is evident that directly it becomes reduced in size, owing to oxidisation, it becomes too small; more battery power must then be applied to overcome the increased R. Again, as every instrument inserted in a circuit adds very considerably to the R of the whole, it is obvious that the addition of a single instrument to a circuit

already carefully calculated cannot fail to be detrimental. But lastly, if a wire be only just large enough for its work in dry weather, or when the line is well insulted, any circumstance which diminishes the insulation—that is, which causes leaks for the escape of the current—will increase the tendency of the current to leave the line wire to a greater extent than if the capacity of the line were ample. This latter point needs some explanation. The fundamental principle which should govern the construction of every electric telegraph is, that the battery current should be compelled to traverse the entire circuit without loss or unnecessary hindrance. Electricity is a swift agent, but even electricity will save itself unnecessarily long journeys when it can. If it detects *en route* a loophole, however minute, it will escape in a greater or less degree, the degree being proportionate to the facility for its escape. This is indeed a popular way of regarding R; the largest amount of electricity will travel by the route on which it meets with the least R. If, then, the conductor be of ample area, the facility it offers for the passage of the current along itself will be greater in proportion to any given fault or faults than if it be only just sufficient; and therefore the divergence of the current from the line will be less.

Glass, earthenware, porcelain, ebonite, are all exceedingly low conductors of electricity, but when their surfaces become moist the moisture conducts. Rain water is pure water, or nearly so, when first it falls, and in this state conducts 3377 millions of times less than iron; it is, therefore, only when the length of a line is such that several thousand insulators, or points of support, occur throughout it, and when all these are simultaneously wet, that any appreciable loss of current takes place. This is supposing the insulators were free from dust when wetted, for if dusty, or sooty, and then wetted, the loss is greatly more apparent. In the neighbourhood of large towns, and especially in the vicinity of certain manufactures, the loss is also far greater, owing to the salts with which the air is charged, and which the water absorbs. The importance of these well-known facts cannot be over-estimated. If the R of the line, and the joint R of the faults be equal, then half the current is lost. The law of electric currents in respect of diverging paths, is that the amount of loss is in inverse proportion to the R of the paths. Upon lines where several conductors are attached to the same supports, the dust and moisture which accumulates, not only upon the insulators but upon the surface of the wood between them, affords a path along which the electricity will creep, if possible, from one wire to others. This it will readily do if the insulators are not kept clean, since tw[illegible]re wires present less R than one. This creeping to ot[illegible] should be prevented as much as possible,

for it will produce upon them signals not intended for them, thereby causing confusion. This is called "contact." It may be entirely removed by attaching a wire, called an "earth" wire, to each insulator support, and letting it pass down the pole into the earth, it acts by being a far less resisting medium than the damp surface of the wood. The wire may be with advantage extended upwards as well, and terminated in a point projecting above the pole; it will then act as a protection against lightning.

There must, however, be a limit to the size of the conductor, and between circuits of greatly differing lengths it is quite permissible to employ different gauges of wire. No. 8 B. W. G. is ample for a circuit of two hundred miles, but it is unnecessarily large for one of two or three miles. A very considerable number of circuits do not exceed this length—for instance, those employed in train signalling—and for them a wire of No. 11 gauge is ample. Upon the other hand, for long through circuits of four or five hundred miles, it is desirable to increase the area of the conductor to about No. 4 gauge. In India, for local reasons, No. 1 is frequently employed. The greater cost of homogeneous iron wire renders it undesirable to be employed for ordinary lines of telegraph, neither is its superior strength necessary except for very extended spans. For over-house telegraphs, where the spans are frequently from two to three hundred yards in length, and where the result of a breakage is serious, it is usual to employ a stranded iron wire. There are both advantages and disadvantages in the employment of a strand over a solid conductor. The strand is, perhaps, slightly stronger, weight for weight, than the solid wire, three No. 18 gauge wires weighing 7·32 lb. per 100 yards, breaking strain 600 lb.; a No. 13 solid wire weighing 7·05 lb., breaking strain 568 lb. The larger surface exposed to the atmosphere is an objection in the strand, but its greater pliancy makes it safer to handle in passing it across streets and over the roofs of houses. The size usually employed now is three strands of No. 18 gauge. Seven No. 20 gauge was formerly used, but the surface for oxidisation is too considerable.

With respect to insulators, their form should be such as—without greatly increasing their size—shall present the longest straight line of resisting medium between their support and the wire resting upon them. The "invert," either single or double, meets this requirement. In addition to the prolonged surface of resisting medium, that part of it underneath the bell will be less subject to the deposit of moisture than that which is exposed. Here there will often be a zone or belt of dry surface when the remainder is wet. Of course this form is no protection against

fog, which, it is well known, will find its way inside the neck of a bottle, but in rain the under side will be dry. The introduction of a metal cap over the earthenware was at one time presumed to be an advantage in lieu of the insulating invert, because the heat which radiated from it quickly dried up the damp deposited upon the opposed surface of the earthenware; the loss of insulating surface was, however, a more serious disadvantage than any gain resulting from its adoption.

Every form of invert has, however, its disadvantages; for since it is impossible entirely to exclude moisture, even from the under surface, and as, whilst moist, dust will be certain to settle, so whilst the first shower will wash all exposed surfaces clean, the under part will retain the dirt until cleaned by hand. The glaze upon the surface is of advantage, as it prevents the water forming a continuous film, allowing it to settle into unconnected drops. Glass is not a desirable material, because it more readily attracts moisture to its surface than porcelain.

The chief causes of fracture in insulators are, stones thrown at them through mischief—an annoyance to which they are particularly subject in the colliery districts—and the unequal expansion between the iron and the insulator in hot weather. To prevent the former, iron caps are frequently employed as a protection at an increased expense and with doubtful success. The latter being more general, is a more serious matter, and it becomes necessary greatly to increase the strength of the insulator in that part into which the iron is cemented, or to introduce a yielding material between the iron and the insulator, a plan adopted in 1855 by the author, who dispensed altogether with a permanent lute, and introduced vulcanised rubber tube. This plan has stood the test of fourteen years with perfect success. Ebonite has been largely employed as an insulator. In itself, ebonite—which may be considered as india-rubber deprived of a large proportion of its hydrogen by the action of sulphur at a certain temperature—is probably the highest resisting medium in existence; but exposure to the sun's light causes the surface to oxidise, and in this condition it becomes porous, and permits the superficial absorption of moisture.

Fig. 1.

Fig. 1 represents one of the early forms of insulator, known as the "cone," attached to the upright side arms of the old square posts by clips passing over the groove in the insulator, and either nailed to the arm, or passed through it, and fastened

upon the inside by nuts. They were threaded upon the wire when it was run out, and in case any became broken after the wire was suspended, some of these cones were made with a slot extending from the outside to the hole. These could easily replace the fractured ones, without the necessity of dividing the wire. Even without the slot, this form of insulator was most inefficient, owing to the very limited surface of insulation, as shown by the dotted lines; but the slot reduced this to less than an inch, reckoning from the top of the groove where the clip passes over the slot.

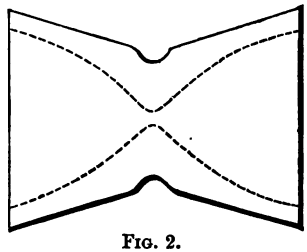


Fig. 2.

Fig. 2 is an improved form of the above, presenting a much longer surface of insulation, and from being very open at the ends, affording a ready means of cleaning the interior. This form is known as the "hour-glass."

Fig. 3 is the radiating invert, a zinc cap being placed over the earthen stem to concentrate the sun's heat, and thereby rapidly to dry off any moisture which may have settled upon the insulator. The limited length of insulation has proved an objection to its use.

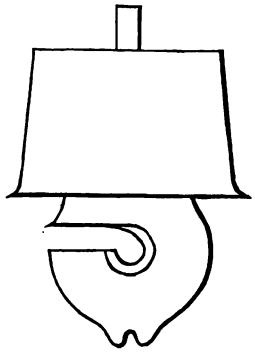


Fig. 3.

Fig. 4 is an ordinary "invert." These have been made of a vast number of different forms, some with a single bell, and some with a double. The space between the bells should be ample, to prevent the accumulation of dirt and spiders' webs, which, when damp, destroy the insulation. The longer surface of insulation, part of which is always under cover, proves of great utility, and the invert, when kept clean, is undoubtedly the best form for an insulator. The iron bolt is fastened into the invert by sulphur, resin, or some other substance easily melted, the bolt and the hole of the insulator being roughed in order to retain the lute.

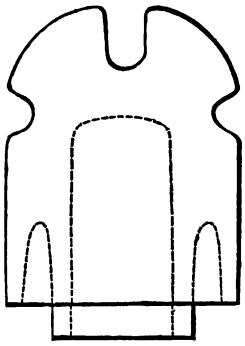


Fig. 4.

Fig. 5 is Bartholomew's invert, in which the bolt is covered with a piece of vulcanised rubber tube, which being

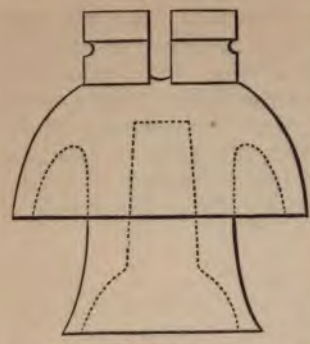


FIG. 5.

strained over the end of the iron retains its natural size upon the top, and forms an elastic buffer, over which the insulator is put. This elastic medium prevents the expansion of the iron in hot weather from bursting the insulator, and in consequence of the invert being merely put over the iron, and retained upon it by the weight of the line wire, it is easily taken off for cleaning, and can be readily replaced if broken, without necessitating the disturbance of the iron.

Figs. 6 and 7 show the double invert shackle, the particular form shown in the figures, being a modification of Mr. Cook's

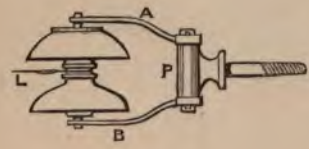


FIG. 6.

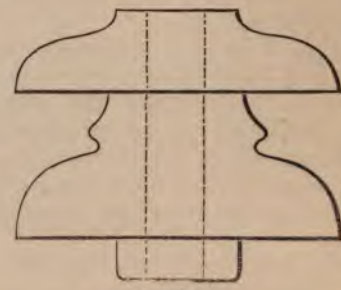


FIG. 7.

old insulator. A bolt is passed through the insulator, the projecting ends passing through two iron straps, A and B, which work upon a strong iron pin, P, which forms the head of the bolt to be screwed into the wood, or otherwise fastened as required. The line wire, L, is fixed to the stem of the insulator between the two inverts. In some cases a second and similar insulator and straps work upon the same pin P; this arrangement, known as Bright's, is convenient for over-house wires, which have frequently to make very acute angles to avoid intervening buildings.

Fig. 8 shows the manner in which the insulator-bearing arms were formely fixed to the posts. Oval pieces of earthenware, C, D, with projecting ends, intervened between the arm and the post, whilst a bolt passing entirely through from arm to arm, kept the whole secure. The bolt was prevented from touching the post by the hole being made considerably larger. This arrangement insulated the arms to a certain extent from the

post, although the amount of insulation between the wires upon the same arm was very limited.

Fig. 9 shows the improved method of fixing the arms. The pole is notched to a depth of about an inch, the cross arm is

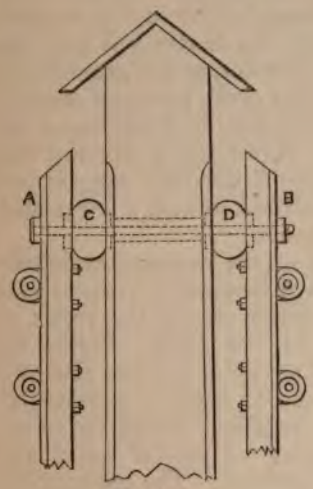


Fig. 8.

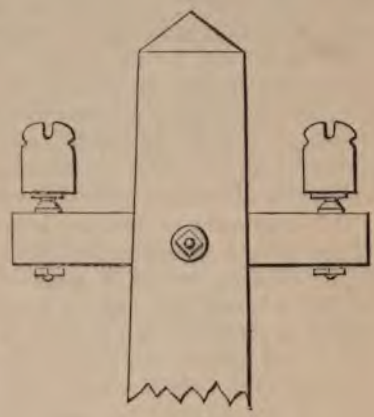


Fig. 9.

tightly fitted into the notch, and a bolt is driven through both the pole and the arm, and fastened with a nut and washer at the back. In all cases where inverts are employed, the risk of a wire falling, when from its crossing a thoroughfare its fall would be serious, is prevented by a guard of wire fastened to the arm and encircling without touching the line wire.

The wooden supports upon which the wires are suspended form a very important item in a line of land telegraph. Originally all telegraph poles were of foreign timber, cut out of the solid balk. In appearance they were unquestionably superior to the rough larch poles so generally adopted now, but their cost was considerably higher, and their durability not proportionate. The extreme difference in the durability of timber placed in the ground has attracted the attention of engineers for a lengthened period; but the telegraph engineer has to deal with the question so largely, and it forms so important an item in his maintenance account, that the matter has had very careful consideration at his hands.

The soil has doubtless much to do with the durability of timber, but the condition of the wood when set has much more to do with it. The butt-end of an English larch pole of forty

years' growth, felled during the winter, and having at least one year's seasoning upon it, will form a good and durable support. In selecting the poles it is best to choose those whose section shows the annulars to be close grown, with a fair proportion of "heart." Injections of creosote, or sulphate of copper in solution, may be advantageous, but if the sap be thoroughly expelled before the surface is painted or tarred, the timber will last very well. Constant attention is needed, however, to the ground line, a foot above and a foot below. At least every alternate summer the ground should be cleared out for a foot in depth round the pole, and the timber allowed to dry. When dry, a plentiful coat of gas-tar should be applied, and the soil filled in; the expense of this most necessary precaution will not exceed a few pence, whilst the advantage gained is very great.

It is necessary to test the interior of the timber occasionally by inserting a strong knife-blade, for it frequently happens that a fair exterior covers a mass of interior decay, the result of having allowed the sap to remain in the pores. Before the timber is placed in the ground it is advisable to char that part which will become the ground line, and to apply to it whilst hot —provided the sap has been entirely expelled—a mixture of one part of Stockholm tar, and four parts of gas-tar boiled with quicklime, to expel the water and ammonia. The ordinary life of a telegraph pole may be taken as fourteen years, but the author has seen poles which have stood for twenty-four years in by no means the last stage of consumption, and he has seen others nearly decayed through which have not been in the ground six years. He believes the cause of this remarkable difference is due solely to having disregarded in the latter case the principles just alluded to, either wholly or in part.

The remarkable facts which have developed themselves in connexion with submarine telegraphy, and which render it so interesting and important a branch of the subject, were first observed in connexion with subterranean wires. In or about the year 1854, it was noticed that upon attempting to send a current through two well-insulated buried wires between London and Manchester, the ends of which were united at Manchester, that an appreciable space of time elapsed between the transmission of the current into the one wire and its appearance at the other wire, provided the earth formed part of the circuit. That electricity should occupy an appreciable period in traversing 400 miles was a new fact; and, indeed, when two suspended or open wires of the same length were united in the same way, the time occupied in the passage of the current was to all appearance momentary; it could not therefore be the distance which caused

the delay. Again, if the buried wires were joined up so as to form a circuit wholly metallic, the earth being excluded, the same retardation did not occur.

The discovery of this fact, its evident importance in connexion with submarine telegraphy, the imperfect state of our knowledge of the laws which govern the phenomenon, a consideration of the immense value of submarine lines of communication when completed, and the great pecuniary losses which had been incurred in futile attempts to lay cables up to 1861, was an ample justification for the appointment of a Royal Commission in that and the previous year to investigate the entire subject. Out of 11,364 miles of cable submerged up to that period, but little more than 3000 miles were in working order, or earning a revenue.

The same laws of R hold good with respect to submerged conductors as with suspended wires, and it is not therefore necessary to repeat them here. Indeed, the law of induction also applies equally in each case, but the conditions necessary to its full development exist in so feeble a degree in the case of suspended wires, that it is not to be wondered at that the effects of induction in connexion with telegraphy should altogether have escaped notice when no other form of telegraph existed.

It may be necessary to offer a few words of explanation upon this point. If a wire, covered throughout its entire length with an insulating material—say, gutta-percha—be suspended upon insulators in the same manner as an ordinary open wire, it will present upon the application of a battery pole no features differing from those which an open wire would manifest under like conditions; but if the same covered wire be laid in water or moist earth, it is found that when the battery is applied in the manner shown in Fig. 10, the galvanometer will indicate that a momentary rush of electricity passes into the wire. This rush is called the "charge," and the wire is then said to be "charged." If the battery be now withdrawn, and an earth wire put in its place, the galvanometer indicates that another rush of electricity passes out of the wire into the earth. This second effect is called the "discharge." Now as no such phenomenon occurs when the insulating wrapper of the conductor is itself insulated, the inference is that the charge and discharge are dependent upon the combination of just two conditions—namely, that the wire shall be insulated, and the insulator not insulated. In another point of view we have the insulator—the dielectric—placed between two conductors, and in this lies the secret of the phenomenon.

Let an insulated wire, C, Fig. 10, be coiled into a tub of water, one end of the wire being free and the other end connected with

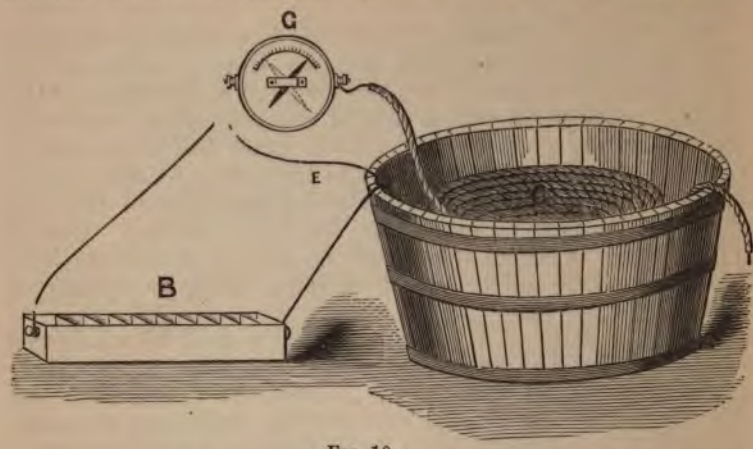


Fig. 10.

one terminal of a galvanometer, G. Now, let a wire, E, from one pole of a battery, B, be led into the water. When contact is made between the other pole of the battery and the other terminal of the galvanometer, a sudden and momentary deflection of the needle ensues, the needle quickly returning to zero. If now the battery pole be withdrawn from the galvanometer, and the wire, E, be substituted, a similar but opposite deflection of the needle occurs. The phenomenon may be thus explained:—The wire dipping into the water may be regarded as the earth wire, and this conveys the tension of that pole of the battery with which it is connected, to the water, and by it to the whole exterior surface of the dielectric. The other battery wire, when connected with the conductor, conveys in like manner the tension from the other battery pole through the galvanometer to the conductor, and through it to the whole interior surface of the dielectric; and it is the passage of this electrical tension into the conductor which produces the momentary deflection of the needle. As soon as the entire surface of the dielectric is "charged" with this tension, no further passage of electricity occurs, and, therefore, the galvanometer needle returns to zero. The tension thus communicated to the two surfaces of the dielectric will remain upon them, even after the battery wires are withdrawn, in consequence of the high resistance which the dielectric offers to the passage of the electricity through itself. But when the wire, E, representing the entire outside surface of the dielectric, is brought into contact with the inner surface through the

medium of the conductor, the two opposite electric tensions neutralise each other, and the passage out of the conductor, or the "discharge" being in an opposite direction to the "charge," the deflection of the galvanometer is in the opposite direction to that imparted by the "charge."

A somewhat more familiar illustration will perhaps assist to make the point clear. If a common Leyden jar, or a sheet of glass, be coated upon each side within a short distance of its edge with metal foil, and a battery of high tension be applied, one pole to one side, and the other pole to the opposite side, the two surfaces of the glass acquire the same tension as the battery poles, which the metal coating quickly distributes over the area they cover, and as the intervening glass is an exceedingly bad conductor—that is, interposes immense R to the passage of the current both over it and through it—these surfaces will retain their respective electrical states even after the battery is withdrawn, until connected together by a better conductor; and when they are so connected, the discharge of this tension passes through the conductor and becomes manifest upon a galvanometer placed in its circuit, the amount of the swing produced upon the needle indicating the amount of discharge. The amount of charge can be ascertained by inserting the galvanometer in the circuit of one of the battery poles. The proof that the charge is an effect entirely distinct from the constant flow of the current caused by any leakage in the dielectric is, that as soon as the surface is charged, the galvanometer needle will return to zero, although the battery contact is maintained.

It has been found by experiment that different dielectrics will take different amounts of charge and discharge; this amount is termed the "specific inductive capacity." It has also been found that the thickness of the dielectric has an influence upon the amount of induction.

To return to this subject as it affects cables, it will now be easy to trace the analogy. The copper wire is one conducting surface, the water or moist earth is the other, and the insulating wrapper is the dielectric. The surfaces of the conductors may be represented respectively in terms of the diameters of the copper and of the dielectric, and, combining all these conditions, we obtain the following formula, which represents the capacity of a unit of length of a submerged and insulated conductor for charge, this being called its "electrostatic capacity":

$$\frac{I}{2 \log. \frac{D'}{D}}$$

in which I is the specific inductive capacity of the dielectic, D′ its diameter, and D the diameter of the conductor. It is obvious, however, that as surface has so much to do with the absolute capacity of a cable, the length of the conductor will exercise a material influence upon the actual amount of charge. This is gained by multiplying the result by the length.

It is natural to inquire what importance is to be attached to the amount of charge and discharge which a cable is capable of giving. The reply will be seen at once from the fact, that upon it depends the speed at which a cable is capable of transmitting signals—its speaking capacity. However large may be the electro-static capacity of a cable, the whole of the charge and discharge which enters and leaves, must enter and leave it through the section of the conductor, and both these operations, when the charge is great, occupies an appreciable time. It would seem from this that by diminishing the resistance of the conductor, giving, that is to say, greater facility for the escape of the charge, we should diminish the time occupied. This is true, however, only up to a certain limit, for it must be remembered that by increasing the diameter of the conductor, we also increase its surface, and that in a higher ratio than the resistance diminishes. A reference to the formula shows that as D increases in value, the entire expression increases also.

The rate at which a cable can charge and discharge itself is proportioned to the square of the length, other things being equal; and the speed of charging and discharging is after all only another mode of expressing the speed of working, because, before a second signal can enter the cable to be effective at the farther extremity, the first must be got rid of, unless every signal, and every interval between a signal, be exactly equal in duration; but as in practice this is unattainable, it is of the utmost importance that every means should be adopted to reduce the induction.

The enormous inductive surface of a long cable is startling. A single mile of No. 16 gauge wire presents a surface of 85·95 square feet, but even this does not represent the extent of inductive surface; and such a wire in a cable 1000 miles in length contains almost exactly two acres of surface. Each of the present working Atlantic cables contains an area of conducting surface exceeding eight acres.

A consideration of the various dielectrics which have been employed in the construction of submarine cables, either experimentally or in actual practice, will prove both interesting and instructive. A large number of substances were submitted to the Committee of Investigation of 1860-61, and a very careful and prolonged series of experiments were conducted, in order to

ascertain their merits and demerits. Almost the whole of them, however, contained either gutta-percha or india-rubber, either pure or in combination with some other material.

They were all subjected to the following tests:

Inductive capacity.
Insulation under changes of temperature.
Insulation under water pressure.
Absorption of water.
Effect of daylight.
Effect of exposure to the atmosphere.

The result of these experiments may be briefly summed up as follows:

The inductive capacity of caoutchouc is less than that of gutta-percha in the proportion of ·8 to 1·0; its insulation specific R is more than twenty times greater; it absorbs more water superficially, but the penetration of the water is not considerable. Both caoutchouc and gutta-percha absorb from three to five times less sea-water than fresh, and the salter the water the less they absorb. The insulation of gutta-percha decreases under increased temperature much quicker than caoutchouc.

The quality of gutta-percha for telegraphic purposes has been greatly improved within the last few years. Its specific R has been nearly doubled since 1864, and the author has seen gutta-percha which had been submerged in water at 100° Fahr. exhibiting but little diminution of R.

In the construction of a submarine cable it is desirable, after having decided upon the core, to make the outer portion—the protecting covering—of such a low specific gravity compared with its longitudinal strength, as that the completed cable shall be capable of supporting a very large excess of its own weight, when submerged in the deepest soundings in the proposed line of route. The wisdom of this precaution will be seen when it is remembered that it is not always possible to lay a cable unintermittently. Many causes may and do continually arise for necessitating the stopping of the paying out, and when this occurs, the cable has sometimes to do the double duty of bearing all the strain due to its own weight, and also to hold on the ship. But more than this: if a fault has gone overboard, it is imperative not only to cease paying it out, but actually to haul in perhaps some miles of what is already submerged. Every one knows or can understand the vast difference of strain upon a cable when being hauled in, and when being paid out. The narrative of the laying of the Atlantic cable of 1865 is worth the perusal of all who take any interest whatever in these matters. It will be found to contain much that is instructive as well as deeply interesting. (See *Macmillan's Magazine*, for September,

1865.) The last two Atlantic cables—those of 1865-66, combined the essential characteristics of low specific gravity and high breaking strain in an eminent degree. The deepest soundings between Ireland and Newfoundland do not exceed 2400 fathoms, or less than 2½ nautical miles, yet these cables were calculated to bear eleven miles of their own length in water.

A comparison of the principal features in the old and new Atlantic cables may be instructive:

Old Cable (1858).	New Cable (1865).
Conductor.	
A strand of 7 22-gauge copper wires, weight per knot, 107 lb.	A strand of 7 18-guage copper wires, weight per knot, 300 lb.
Dielectric.	
Three coats of gutta-percha, weight per knot, 261 lb.	Four coats of gutta percha, alternate with Chatterton's compound, weight per knot, 400 lb.
External Protection.	
Eighteen strands of charcoal iron wire, each strand composed of 7 22½-gauge, laid over hemp saturated with tar.	Ten solid wires of homogeneous iron, No. 13 gauge, each wire covered separately with Manilla yarn saturated with preservative compound, the whole laid over jute yarn saturated with preservative compound.
Weight in Air.	
20 cwt. per knot.	35¾ cwt. per knot.
Weight in Water.	
13·4 cwt. per knot.	14 cwt. per knot.
Breaking Strain.	
3 tons 5 cwt.	7 tons 15 cwt.

In a few unimportant respects the cable of 1866 differed from that of the previous year, its specific gravity was somewhat higher, but this was compensated for by its increased breaking strain. The advantages in the employment of a stranded con-

ductor over a solid wire, when the metal employed is one to which a high test for longitudinal strength cannot be applied, as is the case with copper, is very apparent. If in a single wire a fault occur, or a bad joint be made, complete loss of continuity results from its yielding, whereas in a strand, care being taken that the several wires are never joined at the same spot, if any one wire give way, others remain effective.

The conductor of a submarine cable is invariably copper. It has been attempted to introduce those wires whose function is to take the strain off the copper into combination with it, and so by increasing its area to reduce its resistance, but this has only been done experimentally, and is not regarded with favour. It may therefore be assumed that the conductor is in all cases copper. Its high specific conductivity permits of a small section, and the small surface which this involves means decreased inductive area—a point which has been shown to be of paramount importance. The weight of a copper strand of 7 wires—6 round 1—is as compared with a solid wire of equal diameter, as 54 to 69.

The conductivity of copper varies immensely, the difference being traceable entirely to the admixture of foreign matter. Pure copper being regarded as 100, the addition of 2·45 per cent. of silver—an equally good conductor—reduces its conductivity to 82·52. An addition of 5·4 per cent. of arsenic, a metal frequently found in combination with it, reduces its conductivity to 6·42. Some specimens of Spanish copper, owing to the presence of arsenic and other impurities, conduct no better than iron. The existence of these facts has pointed out the necessity for adopting a standard of R in testing copper for electrical purposes. What this standard is may perhaps scarcely be said to be definitively settled yet. It has engaged the attention of a committee of the British Association for several sessions. The standard of R in former use was equal to one mile of No. 16 copper wire pure, which was regarded as a unit, but the British Association has deemed it advisable greatly to reduce this unit of measurement.

Whatever standard is adopted, temperature must be carefully considered. A rise of 4·4° Fahr. causes a loss of 1 per cent. in the conductivity of copper. Alloys vary very much less under changes of temperature than pure metals, and hence an alloy is the best material to employ in the construction of a standard, or in the making of artificial resistances. German silver, an alloy of copper, nickel, and zinc, is an excellent material for the purpose.

All cable cores—by which are meant the conductor and its insulating wrapper only—both before and after covering them, should be kept submerged at a temperature as nearly that of the

ocean at great depths as possible; this is 39° Fahr. By the adoption of this precaution, faults occurring during the manufacture are at once detected. The desirability of keeping the completed cable permanently submerged is shown from the fact that the Malta and Alexandria cable of 1861, covered with 18 No. 11 iron wires, was coiled after completion into leaky tanks, the water being occasionally renewed. The iron sheathing was therefore alternately wet and dry, and the result was such a rapid oxidisation of the metal that it generated heat to the extent of 86° Fahr. Such a result should be carefully avoided, because, if gutta-percha be employed, it becomes soft at high temperatures, and if in this state the core is bent, the copper strand is liable to become decentralised, even if it does not, as has more than once occurred, altogether protrude from the gum. The uncovered core of a cable should never be subjected to a high strain. If stretched, the copper will permanently elongate, whilst the percha will to a certain extent return to its former length; the result of this is that the copper will buckle, and alter its position. If the outer wires are on, the elongation is prevented to any serious extent.

With respect to the elongation of iron wire, it may be mentioned that No. 8 gauge will vary from 10·5 to 11·16 per cent. before it breaks. The softer the wire the more it will elongate, and the less strain it will bear.

Subterranean wires, it has been shown, being under the same laws of induction as submerged wires, manifest the phenomenon to an equal extent. The conductor is always copper, covered either with gutta-percha or india-rubber, and protected outside usually by strong tape laid on spirally and passed through tar, and plentifully sprinkled with sharp sand before the tar solidifies. The sand adheres to the surface embedded in the tar, and forms an admirable protection to the whole. If the wire in this state be drawn over a brick, it will saw the brick in two.

It is usual to protect buried wires when laid under streets by drawing them through iron pipes, but, in country roads, a stout wooden guttering may be sufficient. An economical plan, and one which answers well in some localities, is to take out a trench about 18 in. deep, and to sift a portion of the thrown-out soil back into the trench, then lay in the covered wires and sift over them another portion of soil, laying upon this the larger and flatter stones, over which the remainder of the soil may be thrown and rammed in.

The advantage of test boxes at intervals along a length of buried wires is doubtful. They may and do present facilities for dividing the circuit without breaking the wire; but every

test box is more or less a source of leakage, a loss of insulation. Ground test boxes are decidedly objectionable; upright boxes are better. The position of a fault, not of continuity but of insulation, may be very closely calculated, provided the elements of the core are well known previously. It resolves itself into a question of R, which is proportionate to distance.

A few general remarks upon the submergence of cables may be instructive. This is an operation in which too much care cannot be taken. Remembering that whether the operation be a long or a short one, every moment of the time the cable is going overboard is a moment fraught with risk to the enterprise, it is evident that no precaution should be omitted, the neglect of which would endanger the cable. Every means should be adopted to prevent a stoppage in the paying out, or even a diminution in the speed of the cable, and the deeper the water, the more urgent is this precaution. Great attention must be paid to the dynamometer, so that, the breaking strain of the cable being known, an ample margin may be given the breaks. But, after all, no positive rule can be laid down for the paying out of a cable. A man of ability will adapt his arrangements to the requirements of the situation, which will vary with weather, depth of water, and casualties which may, and indeed must arise. A general rule may, however, be laid down which applies particularly to great depths of water, which is, that the safety of the cable when being payed out is increased by an increase in the ship's speed, provided always that the staff of cable men can keep pace with the speed at which the cable travels. The more rapidly a cable leaves the ship, or rather the more rapidly the ship leaves the cable behind, the greater will be the angle between it and the perpendicular over the stern; the cable lies more horizontally upon the water, it sinks less rapidly, and requires less pressure to be applied to the breaks. These are self-evident conclusions.

The successful laying of cables in 2400 fathoms, and, above all, their successful recovery in like depths, affords a sufficient proof that no difficulties, not surmountable, need be feared in the laying of any future cables in fathomable depths; but when we come to speak of the permanence of cables, however successfully laid, the aspect of things becomes changed. No guarantee can be given in this respect.

The character of the ocean's bed varies immensely. In the Mediterranean, the unevenness and inconstancy of the bottom in the volcanic area has proved singularly fatal to the continuance of cables. On this account great judgment is needed in the selection of a suitable route by careful previous soundings.

The bottom of the Atlantic between Ireland and Newfoundland proved to be eminently adapted to the reception of a cable. For 28 degrees of longitude the depth of water varied, with very trifling exceptions, from 2000 to 2300 fathoms, and the bottom, with equally trifling exceptions, consisted of a species of ooze, found, upon a microscopic examination, to consist of minute shells. The presence of these shells has been regarded as a satisfactory proof that at the great depths of the ocean the water is in a state of rest, and that any cable being once laid there will remain undisturbed. The landing-place of a cable is, however, a different thing altogether. However strong and well protected in itself the shore end of a cable may be, it cannot withstand the incessant beating of heavy waves upon a rocky coast. The Channel Islands telegraph cable is a remarkable instance of this fact. It may often be advisable to deviate considerably from a direct line in order to secure a better and more sheltered spot for the landing. But it is not only close to the shore that a cable is in danger. Even in water of from 100 to 200 fathoms a cable is not secure from the abrasion of icebergs of high specific gravity, and there seems little room to doubt that the only accidents which have occurred to the present Atlantic cables are due solely to these monsters.

A rapid tide-way should be avoided for the bed of a cable; the rust, which unavoidably forms upon the external wires, instead of accumulating and in time preventing further corrosion, becomes washed off, until the entire wire is gone. In some situations cables become silted up with sand and shingle, and cannot then be raised for repairs. The adhesion of weed, coral, and other marine organisms is detrimental to a cable as facilitating its corrosion. The cable across Bass's Straits was actually floated by the mass of weed which enveloped it, although this cable weighed two tons per mile. Lastly, as far as possible, any frequented roadstead should be avoided, for a ship's anchor is a deadly enemy to telegraph cables, and all notices as to anchoring will stand for nothing in a gale of wind, setting on shore. The only remedy for this, if such a locality cannot be avoided, is to take the bull by the horns, and put down a cable over which any ship of ordinary tonnage may ride with the risk rather of breaking her own rope than the telegraph cable.

DISCUSSION.

Mr. Nursey said that one point did not appear to him quite clear. In the paper, the author stated that the deepest soundings between Ireland and Newfoundland did not exceed 2400 fathoms, or less than 2½ nautical miles, yet the cables were calculated to

bear eleven miles of their own length in water. He (Mr. Nursey) believed it did not matter what the depth might be, it was simply a question of the distance between the points upon which the cable might take a bearing, and he presumed that if the depth was double that stated by the author, the breaking strain would not require to be greater than that named. He assumed that the breaking strain was calculated simply to guard against the interval which might be found between any two points upon which the cable found support. He believed that in all the Atlantic surveys it was found that two such points were not in any case more than nine miles apart, but eleven miles were taken as a maximum, and to this maximum the breaking strength of the cable was proportioned.

Mr. Olrick said that generally telegraph cables were covered externally by iron wires laid spirally, and when any strain came upon a spiral it would elongate, and in the same proportion as the spiral elongated the strain would be thrown on the internal core, or the conductor. Some years since he devised a means of overcoming that difficulty. His arrangement was to have an elastic core and a copper conductor arranged in a spiral around it in such a manner as that to whatever extent the external spiral might stretch, the internal spiral conductor would stretch in exactly the same proportion, and that it would have the power of returning to its original position. He, however, found that he had been anticipated by a previous inventor. He would ask the author whether he did not consider the invention of value in connexion with ocean telegraph cables. The author stated that the quicker the speed of the ship in paying out a cable the better, but it appeared to him (Mr. Olrick) that if there happened to be a deep valley between two sharp rocks, the quicker the cable passed over them the more strain there would be upon it, whereas, if a cable had time to adjust itself to the sloping sides of the rocks there would not be so much liability to damage the cable from the sharp edges of the rocks. He would ask Mr. Bartholomew whether a conductor had not broken on account of the sudden strain brought on the copper conductor by the stretching of the external spiral.

Mr. Nursey said, with regard to the sharp angles of rocks at the bottom of the Atlantic, to which the last speaker had alluded, he did not think that they existed, as the bottom of the Atlantic was undulatory. But even if such angles were met with they would not have a very material effect upon the cable, which would, of course, have sufficient weight in the length between the ship and the bed of the ocean to cause it to adapt itself rapidly to any irregularities. The slow motion of the ship

would probably cause too much cable to be payed out, and would give rise to loops and kinks, and ultimate damage.

Mr. BERNAYS asked, in reference to overland telegraphs, whether the strength and quality of the wire used had any effect on its conductivity, and whether electricity passed at different rates in a wire of great tensile strength, and in one of small tensile strength. He also wished to know whether iron poles had ever been used for supporting telegraph wires, and whether they had any effect on the passing current?

Mr. NURSEY said, that although the author had confined himself to the subject of telegraphy irrespective of apparatus, it was so excellent a paper that he hoped in the course of time they would be favoured by Mr. Bartholomew with a sequel to it in the form of a paper on telegraphic apparatus. The question of apparatus was at the present time one of the most important, especially to many members of the Society who were connected with railways. On the first of April next there would come into operation an Act requiring that all railway trains should have means for enabling the passengers to communicate with the guard. He had examined many schemes for the purpose—mechanical, electrical, and pneumatic, and it seemed to him that the electrical schemes were the best suited for attaining the object. An electrical arrangement was working very well on the South-Western Railway and other lines.

Mr. BARTHOLOMEW, in reply, said that he did not apprehend that at the bottom of the Atlantic, at least, there were any parts having such sudden dips that a cable would be suspended for any considerable length, if at all. Taking the most mountainous district known, if a cable were coiled upon a drum and paid out from a suitable conveyance, the cable having on it the same amount of strain which was uusally put upon telegraph cables when being submerged, and with an allowance of 15 per cent. of slack, he apprehended that the cable would follow very closely the unevenness of the route. He thought that a telegraph cable being submerged along the bottom of the ocean would follow the same course. No doubt there were—and certainly in the Mediterranean—very peculiar formations at the bottom; but when he spoke in the paper of the soundings of the Atlantic between Newfoundland and Ireland not varying more than about three hundred fathoms, he, of course, did not mean that there was a sudden difference of three hundred fathoms. On the contrary, every one who had seen a section of the bottom of the Atlantic—as developed by the deep sea soundings taken for the purposes of the Atlantic cable—would know that even in the case of a certain place about 300 miles from the Irish coast,

where there was what was described as a sudden dip from 1750 fathoms to about 550, and which was regarded as a great difficulty in laying the Atlantic cable, that difference was spread over a length of about ten miles; it would therefore be evident that no danger need be apprehended from such a slope as that. With regard to Mr. Olrick's question as to the spirality of the outside of cables, the difficulty was anticipated to a certain extent, and there were on the table several specimens of cables in which there were no spiral coverings. In that case there was nothing to untwist. The wires which actually took the strain were the longitudinal, and therefore the idea of strain on elongation in the case of a spiral was met there. But the difficulty was not one which was at all experienced in practice. It was found that the amount of strain which was put upon cables in paying out was not sufficiently serious to elongate the whole fabric. He thought that the inequalities of the bottom of the ocean were rather overrated by Mr. Olrick, and that it was quite a wrong impression that the bottom of the Atlantic consisted of vast ravines. No doubt such places did exist at the bottom of some ocean, but the deep sea soundings in the Atlantic showed the contrary. With regard to Mr. Olrick's question as to the copper conductor breaking, he (Mr. Bartholomew) had no doubt that copper, used as a conductor, both in the solid form and the stranded, had broken, though he did not look upon that result as connected with the simple elongation of the copper. Copper would elongate greatly more than iron. If such breakages occurred, they were probably owing to faulty places in the copper, and it was to meet such defects that the strand had been adopted. He believed that if the cause of certain conductors having broken was traced to its source, it would be found to be not simply due to the elongation of the cable. He was not aware that the quality of iron with regard to its tensile strength had any bearing on its conductivity. Iron, if pure, would conduct equally well whether it had a high or low tensile strength. He did not know that there was an essential difference between the conductivity even of hard wire and of soft wire. With regard to iron poles, they certainly were beneficial. He had not touched upon the point because he had not had sufficient experience in the use of iron poles for telegraph purposes, and in fact there had been very little experience in the matter in this country. He believed they were used to a certain extent on the Continent, for instance, in some parts of Germany. It must, however, be borne in mind that an iron pole would cost at least double what a wooden pole would cost, and the experience of their use had not extended over a sufficient

time to show whether their double, or even treble expense, would be compensated for by a proportionately greater durability. An iron pole standing 15 ft. out of the ground, would cost about 20s., while a wooden pole of the same height could be obtained for seven or eight shillings. A good wooden pole would last about fifteen or sixteen years, and hence an iron pole to be equally cheap would have to last about thirty years, and quite as much would have to be spent on an iron pole as on a wooden pole for maintenance and repair. As regarded Mr. Nursey's suggestion, that he (the author) should read a paper on Telegraphic Apparatus, he should be happy at some future time to bring the question forward, for it was the most interesting part of the subject.

March 15*th*, 1869.

F. W. BRYANT, PRESIDENT, IN THE CHAIR.

THE PREVENTION OF LEAKAGE IN GAS AND WATER MAINS.

BY C. M. BARKER.

As the subject of the present paper may be deemed of public importance by some, and unimportant by others, the following extracts will explain why it has been introduced to the Society. The first extract is from Mr. W. H. Lefeuvre's inaugural address in 1868 to the members of this Society; the next is taken from the annual summary of the *Engineer* newspaper; and the last is from the address by Mr. J. G. B. Barlow to the Gas Managers' Association.

Mr. Lefeuvre thus expressed himself on this subject in his address:

"The lighting of our cities and towns is a subject which, I may say, to a certain extent affects many of our members, as this Society includes some of the leading gas engineers; and it is of singular importance when viewed in connexion with the exhaustion of our coal fields, as we are told that in lighting our metropolis alone there is an annual loss of 100,000 tons of coal; this loss being occasioned by leakage from the mains, and in various other ways."

The *Engineer*, in its summary for the year 1868, referring to some of the wants of the period, states:

"That in the distribution of gas two methods are employed in making joints. In one the pipes are turned and bored, and in the other they are made tight with lead and the set hammer. Each system has its advocates; but can no improvement be made on those plans? Clip joints have been tried, and found wanting. Nearly all the plans tested are supposed to possess the advantage of enabling a pipe to be removed when required

by merely unscrewing the bolts which hold the clips together, or by displacing a wedge. But, unfortunately for such assumptions, after the joints have been in the ground for a year or two it is impossible to disconnect them without breaking the joint proper. Bored and turned pipes have been used in Liverpool during the lifetime of one of our most celebrated gas engineers in England, and the system has been satisfactory; but where unusual care has not been taken in setting such joints there has been great leakage. Can no plan be devised of making a joint less rigid, and with greater certainty of being tight, than those which are bored and turned, or the ordinary lead joint? We think such an improvement is possible, and there is ample scope for exercise of ingenuity in this direction. Leakage is always felt to constitute a serious item in the distribution of gas, varying as it does between 10 and 30 per cent."

The President of the British Association of Gas Managers—Mr. J. G. B. Barlow—observed that the present joint for street mains offered a wide field for the exercise of ingenuity.

In thus alluding to these authorities it will be apparent how necessary it is that this loss should be prevented, if possible; and therefore, to place this subject clearly before the meeting, it will be requisite to state the probable causes by which gas escapes from the pipes, and how a loss ensues upon the quantity manufactured.

The loss experienced in conveying gas to the consumers arises from other causes than that of leakage, one of which is from the condensation that takes place when the gas passes through pipes which are at a low temperature. This is dependent, to a considerable extent, on the nature of the soil in which the pipes are laid. A further loss is occasioned by an exosmose quality which all gases possess, of a diffusive nature, penetrating porous bodies to assimilate with the air contained in the pores of any material that may be used for holding or conveying gaseous fluids. It is by this quality that gas penetrates and passes through cast-iron pipes, when it is under light pressure, and is thus extracted as by an exhaustive process to mix with the air on the exterior of the pipe. This quality appears to be reversed when the gas is under a heavy or ordinary pressure, as during the time it is being burnt an endosmose or an absorbent action ensues, and air from the exterior of the pipes passing through the iron and mingling with the gas is consumed, to the detriment of its illuminative power, as the absorbed quantity of air is greatly in excess of the gas abstracted. The addition must be taken in [illegible]ensation of part of the loss occasioned by condensation, [illegible]ill leave nearly the whole loss to be accounted for by [illegible], which has been proved before a parliamentary com-

mittee to amount to not less than one-sixth of the whole quantity manufactured. This confirms Mr. Lefeuvre's statement that the waste of coal is yearly, in London alone, to the extent of 100,000 tons, which, if manufactured into gas, would realise 150,000*l.* for the metropolis alone, where the losses are about the average. If provincial cities and towns are proportionately wasteful, the annual sum will be increased to so large an amount as not easily to be estimated, but it is a sufficient sum in money value loss to call general attention to such waste, and it ought to be worthy of legislative interference on behalf of our sanitary condition, as it must be most prejudicial to public health that such an immense quantity of gas leakage should continually take place. The escape of gas renders even our public footpaths dangerous when cellars are beneath, and our institutions and public buildings insecure, whilst there is no supervision for remedying the fearful evils of defective mains, which are quite independent of public authorities to control or prevent, as proved by the recent accident at the street semaphore near the Houses of Parliament. Had the result of that accident similarly burnt and affected an important State officer instead of the unimportant police officer on duty, something more, no doubt, would have resulted from it than merely probing the soil to disperse the escaping gas from the adjacent leaking mains.

With the knowledge of the existence of leaking mains, and their concurrent evils, engineers have long been familiar, and the specifications of many patents show the numerous ingenious methods that have been tried for preventing the ceaseless waste that is ever accumulating by the present defective system. Were the object but to seal the pipe joints, some of the existing patents would effectually prevent leakage, but there are other conditions to be complied with prior to the adoption of any new system of mains by gas companies. The first, and paramount, of these conditions is that of cost; for the sum paid for new pipes must not be an increase on the ordinary pipe, or the use of the latter will continue with its defects. It is well known that ordinary pipes are stock articles of trade, and being required in large quantities, close competition has brought their manufacture to the lowest remunerative profit. Any new pipes must possess quality to stand the pressure test, and be cast vertically to prevent defects and blow holes. They are compelled to be made by unskilled, or cheap labour, to be saleable at the prices charged for them, they must have no extra metal for any special purposes, for thereby the price will be increased through the weight. To these are added some secondary conditions, divisible into the materials for sealing the joints, and in the method of

laying the mains. For the first, the sealing material must be inexpensive, and it must be reliable, and also it must not deteriorate the pipe. The material must be easy of separation when the joints are required to be broken for displacing and relaying the pipes without injury to the sealing surface or general effectiveness. The pipes must have a simple form in their arrangement for jointing, so that the main-layer may quickly place and seal them; and there must not be any adjuncts to the pipes, because such would occupy his time by fitting together. No extra depth of trench or greater width of excavation will be allowed; and it is under such conditions that all new pipe systems must pass prior to obtaining a favourable consideration, and before the adoption of any new mains for preventing leakage, they have all these difficulties to contend with, and they must be surmounted. So it is easy to agree with the expressed verdict of the *Engineer*, that all the inventions for this purpose have been found wanting, and had the conditions been made but a little more difficult, the solution of this problem would have nearly equalled that of discovering perpetual motion. Independently of the requirements alluded to and their difficulties, the prospect is not enticing as a profitable investment, for the most remediable system can only expect adoption by the slow growth of extensions into fresh districts, or by newly formed gas companies in laying their mains. The cost of new mains is a most serious expenditure to a gas company, and will never be entertained by them whilst the present pipes convey the material to their customers, although the leakage may be very large in proportion to the quantity made—for these reasons it may be that no improvements have been made in gas mains that have been adopted. Further, the price allows but so small a profit to the manufacturer, as to disincline him to any change, and especially as the difficulties of satisfying the requirements of gas companies are so great.

Of the most probable causes that produce leakage in mains, none are so easily accounted for or so readily understood as the negligent and defective work of the "main-layers," who generally work under contract of a price per yard, and are said to "stint the lead;" this work may be done too cheaply. But the result is not so dependent on practice as it is generally believed to be, for when the joints have been most skilfully made with lead and tested, they eventually leak, and the evils of the system fall on the workman, because no better means have been introduced to obviate the loss in continuity, and the inconveniences that ensu[illegible]sing the present pipes; the leaking joints of which [illegible]e admitted for the advantage of its yielding connex[illegible]llows the pipes some play in each length.

The trench for receiving the mains is dug of a uniform depth, and consequently it follows the varying levels of the locality. This is no doubt a primary cause of considerable leakage, as the requisite parallelism of the inserted pipe into a socket cannot be obtained, being prevented by the rising or falling of the trench, and also by the requisite falls for draining the pipes to the syphons. To remedy these deviations the earth is brought up to bed the pipes, but it is insufficient, and causes leaking pipes when the earth consolidates.

The operation for jointing pipes commences by loosely inserting the spigot end of each pipe into the socket of another, and so several lengths of pipes lie in a line upon the "hard," or roadway out of the trench. The workman then places round the inserted pipe some twisted spun-yarn with a caulking iron, compressing it into the annular space between the exterior surface of the inserted pipe and the interior surface of the socket. This gasket usually occupies about two-thirds of the depth from the root of the socket, closely filling the annular space. This operation is expected to bring the pipe inserted concentric with the socket, or if not quite so, it is intended so to be placed that an equal quantity of melted lead may flow round the pipe, and fill the annular space left by the gasket. If the pipe is not concentric with the socket then an insufficient quantity of lead is found in some part, and probably a bad joint is the result. The placing of the yarn is proceeded with in all the lengths that lie in the road, and for facilitating the work the pipes are rolled over to get round the joints. In this part of the operation an evil, doubtless, must arise, as the pipes are dependent for keeping their position one within the other on the caulked gasket only, and must mainly rely on this for resistance to motion given to turn the length of pipe. The gasket thus acted on expands from the compressed form obtained by use of the caulking iron, and thus commences one of the causes that ultimately result in leakage, through the caulking material having been shifted, and caused to get "windage." When the pipes have been filled with yarn a temporary band of clay with a gate is placed round the surface of the inserted pipe, and also adheres to the vertical surface of the socket's mouth. Into the gate, or opening to the clay channel, the melted lead is poured, and fills the annular space between pipe and socket with a cast ring of lead, which in cooling contracts at $\frac{5}{16}$ in. to the foot, so that this lead ring, or collar, shrinks on the surface of the inserted pipe, and there mostly makes a good joint. But the exterior diameter of this lead collar shrinks from the interior surface, and by contraction there is left a leakable space round the lead. This is attempted to be remedied by the use of a blunt caulking

tool beaten against the surface of the lead, "upsetting" it, to give it lateral expansion to fill the space left by contraction. This must be done with care, as the ordinary pipe socket is liable to break off its configuration, causing it to be weak at the root.

If the direction of this force is considered, what must be the most probable result? Instead of a lateral expansion of soft lead there is a drifting inwards of the material, and the effect of each blow on the metal is absorbed in the elastic gasket packing. If any greater force is applied, it serves only to loose any hold the shrinkage of the lead has on the inserted pipe. A light blow is but of slight service for the intended purpose, as the lead when so set up merely drifts up the edge of the metal, and it causes no lateral expansion of the lead. Should such metal caulking be not thoroughly completed round the joint, there will be a space at which the gas can escape, hence leakage must ensue, and the lead be a defective sealing of the joint. Besides the evil caused by the contraction of the melted lead in the socket, there are other causes equally difficult to contend with in this part of the work. One of these results from the melted lead at 620° of heat being poured into the channel bounded by the spun-yarn and moist clay. The melted metal comes into contact with the yarn saturated with tar or grease, and from either of these vapours are eliminated at as low a temperature as 250° to 350° of heat; and at 212° the clay parts with its moisture as steam. Thus these vapours are quickly generated during the pouring of the lead into the gate, cautiously done so as to permit the vapours escaping at the gate. Yet for all the care that can be taken the whole will occasionally burst out explosively; and even when this does not occur the same cause produces "blow-holes" and "honeycomb" in the cast lead, by the confined vapours. Leakage often arises through the lead being unsoundly cast, a defect which is dependent on the pouring of the fluid metal. When the lead, in its descent, courses down the surface of the inserted pipe and meets with any impediment, its channel is diverted into divided streams, these quickly cool, so that on the fluid rising round the pipe and meeting these cold pieces of set metal, it has not sufficient heat to remelt them, and so it flows over the cold metal, and then ensues a "chill cast," by which another unsound casting is produced, forming a defect which will permit the gas to escape. As all the joints in a length of pipes are separately being lead-made on the road-way, t[illegible]hs of pipes are again rolled over, backwards and forw[illegible]e readily caulked with the pliant lead, and to obs[illegible]oints appear sound. It is during this part of the wo[illegible] lengths of pipes are rolled over, that by their

weight they are compelled to adapt themselves to the surface of the road, whatever its form may be, so each turn must alter the form of the lead in the socket in which the spigot is encased, as the road may have either a convex or concave section. Every joint is in some degree strained, altering the lead and gasket packing from the form and position it originally had when first set up with the caulking iron. The result of such strains produces channels for leakage in the pipes so joined; they are afterwards lifted or rolled into the trench, either of which compels the length of pipes either to sag or hog, and so ensues another cause by which an alteration is made in the form of the cast lead as originally run. Further, a similar result takes place on the pipes being placed in the trench; there they are again compelled to adapt themselves to the uneven surface and varying levels of the trench, and for drainage inclination as well as to the course of its tortuous direction, so that under these several operations the original form given to the lead has been altered, sufficiently to permit the escape of gas if the lead was there by itself without the spun-yarn.

The length of pipes is lastly connected with the pipes which have already been finished and laid in the trench; this one being a connecting joint it is made in the trench, and is liable to all the defects already stated as to pouring the lead, and to further defects in leading from the incapability of inspecting the under portion of the joint as it is close to the surface of the trench. In commencing to make this joint it is important to get the pipe inserted centrally in the socket of the pipes already laid, for levels may oppose the admission, and the inserted pipe may not be nearly parallel with the stationary socket. To remedy this it is usual to drive a half-round taper drift-wedge between the socket and pipe, to keep the pipe central in the socket, the wedge deflects the pipe in its length in proportion to the angle. To caulk this joint lengths of yarn are cut to come round as far as the drift allows, and when sufficient in quantity the drift is extracted, the deflected pipe then partly resumes its angular position, nipping the gasket. After this another piece of yarn is inserted over the space previously occupied by the drift, and is ready for the lead to be run round the joint. By this last operation a whole length of pipes is finished for conveying gas, except that they have not been tested (and it is seldom they are); and although all these joints may possess any of the deficiencies stated, yet on application of the air-pressure test they mostly appear quite sound. The excavated earth is then returned, covering the pipes, and they are believed to be free from leakage; but a short time afterwards on opening the soil for making service connexions, the pipes show that leakage has already

commenced, by the offensive smell emanating from the earth, and a light applied to the gas ignites it at some of the joints. One of the causes of such leakage is that the hydrocarbons contained in the gas liquefy the tar or grease incorporated with spun-yarn, the capillary attraction of which holds the solvents, till no vestige is left of the material it originally contained, and thereby materially assisted to keep the joint sound during the pressure test. The adhesive tar and elastic spun-yarn in lead-made joints, are more serviceable than the lead for a seal to the joint, the lead being only useful to confine the sealing substances. The yarn, when denuded of its adhesive qualities, perishes from the chemical action of the bisulphuret of carbon, and then its elasticity is lost so as not to fit closely to the surfaces of the pipe it has sealed, the gas passes through and around it, as also past the strained lead joint to the soil, the moist porosity of which has great affinity for the gas, precipitating the carbon, as the black earth always shows when an opening is made near to a leaking joint. Thus, the gas is condensed, and an exhaustive action ensues which rapidly supplies fresh gas for further chemical decomposition, and so from these defects of the system the loss of gas becomes considerable.

Such are the causes from which most leakages arise where this description of pipe is laid in ordinary level localities, but in hilly districts the evils of leakage are augmented in consequence of the pipes, with the loose soil which has been filled into the trench, having a tendency to descend or slip downwards, from the weight of the upper pipes causing a thrust on those at the lowest end of pipes laid upon an incline. This so affects the pipes as to give them a longitudinal curve, and by their conforming to this alteration they curve in their whole or part of their length, and each leaded joint is compressed or expressed on its inward and outward radii, and from this cause they are made defective, for the security of the joint, being only preserved for a time by the adhesive or elastic properties of the yarn; when that perishes, or the tar is dissolved, the pipes leak almost at every joint. This was exemplified in one of the hilly gas-lighted districts where the colliers used for sport to ignite the gas escaping through the soil over every joint on an ascending hill. Besides the slip of pipes the augmentation of leakage in such districts arises from the high pressure on the mains, as the loss is always increased proportionately to the pressure; this evidently proves that the loss must be greatly attributable to defective joints more than to other causes. As a further proof of it being so, as a remedy for preventing the leakage by a shifting of pipes on ordinary levels, as also on steep ascents or desc[illegible] asonry or stone slabs are now placed beneath the

joints to give them stability and support under any of the causes that occur to displace the pipes. This addition to the usual cost of mains as a precautionary arrangement must prove the defectiveness of the system, and should exonerate the workman who has to make joints from blame. There is another method not generally used for jointing ordinary pipes with what is termed an elastic joint; this is made with gasket packing at the root and mouth of the socket, and the intermediate space between them is filled with Russian tallow.

The other system in use for gas mains is that of turned and bored pipe joints, of taper form. The cone spigot is turned for fitting into a conical bored socket, and when these surfaces are accurate the joint is sealed by the addition of paint. Such pipes are stated to be a better means of securing the joint when unusual care is given to them in laying; but when laid without such care, then great leakage has resulted from their use. These pipes are more costly than the ordinary pipe, as they require additional labour, after they have been cast, in turning and boring. This extra charge may be taken as an equivalent in cost for the lead and yarn used in ordinary pipes, with the additional labour attendant on laying them. It is on the truth of the turning and boring that the value of such joints for pipes entirely depends. That their value in that respect may be fairly judged, it will be well to consider the method adopted for turning and boring them. To accomplish this three or four of these pipes of the smaller sizes are placed on a stationary bed, and there securely cramped down. Near the ends of these pipes are fixed head-stocks, in which revolve poppet mandrils with bosses and chucks, and they carry hard steel cutters. These are set at an angle necessary for turning and boring the spigot and socket simultaneously and concentrically; the mandrils are forced out of the head-stocks against the surfaces of the pipes to be turned or bored, and thus the one is turned and the other bored by the cutters for a sufficient depth as gauged by the protruding and revolving mandrils. This work is performed on cast iron with a surface almost as hard as glass, and with not a little sand, so that the cutters suffer considerably, and soon lose their cutting edge. The cutters are then hand-held to the grindstone for restoring the edge.

On this being properly effected, the surface line of taper is dependent; and therefore, as the surfaces of a grindstone will ever give varying surface to the material so held and ground, these pipes, when bored or turned, take the line of edge of the cutter, which may be ground concave. Yet it is expected that these pipes will make a sound surface jointing, perhaps having for that purpose two concave surfaces meeting. When such

form of surfaces meet, it would require a great amount of "unusual care" in laying to prevent their leaking, which no amount of sealing paint would insure. It is true that the defective means of any procedure in manufacture should not condemn a principle, because, if accurately made, then these pipes would not be liable to this objection. But their manufacture is affected by the price at which they have to be made. This description of work is incapable of being supplied, except by labour and attentive supervision in making true surfaces. If any two of these pipes as ordinarily made are taken, and their surface proved by slightly grinding one within the other, it will soon be apparent that the spigot and socket are hardly ever found to make true and corresponding surfaces so as to join without leak, unless driven in with force that, by expanding the socket, the surfaces touch at some points by being forced together, paint filling the non-touching spaces. If this was not the case, how could they be usefully adaptable for altered levels of any deviation, except by deep excavation and with great deflection of the pipes in their lengths? For any obliquity of line, they are almost inapplicable, except by leaving leaking openings.

The larger-sized pipes for turned and bored joints are made in a self-acting lathe, and they revolve in a cone plate, and the socket is bored with a tool and rest, having a set angle for boring the taper cavity. The spigot end is turned generally in a separate lathe, to which the pipe is taken, and the cone spigot there turned to a similar set angle on a plug centre. For boring the interior surface of the socket, its rough exterior surface revolves in the cone plate surface. If the casting is not of perfect form or even surface, such inequalities will give an obliquity in boring the taper surface in the socket, and the temporary cone plug by which the spigot end is centred and turned is also liable to give the pipe an eccentric line of surface (from the irregular surface of the casting) to that at which the socket has been bored, rendering the possibility of a joint in a right line most unlikely. It is upon the true line of joint with a similarity of cone surface, that the possibility of a good joint depends, which neither of the methods of manufacture described are likely to accomplish accurately. These pipes are laid in the trench, and on inserting the spigots into the sockets with fresh paint on the turned surfaces the joints are closed by driving the spigot of the pipe hard into the socket with a wooden mallet. That done, the joint is completed. Turned and bored pipes have no provision for guarding against leakage resulting from any disturbance or slipping of the pipe that may be caused by deflection or concussion on the pipes from weights passing over the mains or from additional materials contracting the upper surface to a yielding subsoil.

They are also without any provision for making good any accidental fracture, or for taking out and substituting a new length of pipe. Recourse must then be had to lead jointing for repairs; they are most defective for taking up and relaying, as their turned surfaces are more or less oxidised, and they are less fit, from this cause, for remaking a surface joint. Under such altered circumstances of impaired surface it is almost impossible to make further use of them. They are therefore defective as being inconvenient in adapting themselves to the bottom of the trenches from the unyielding character of the jointing, and in an undulating locality they are not reliable, from the necessity of keeping the pipes with a concentricity of joint, which is often impossible. When these pipes are laid in a trench, its inequalities have to be made good to the pipe. This is done by placing the loose earth beneath the pipe, and this yields on settling; the pipe has thereby an allowance for extra deflection, and this draws the pipe from its joints at both ends, and results in considerable leakage, because the space is left round the diameter of the socket that is not occupied by the spigot, and hence ensues the cause why the leakage of turned and bored joints has been great. Whatever unusual care may be taken in laying this description of main, it can never be other than a most expensive system, even if they are ever made with the perfect accuracy of surface necessary for making a sound joining. In Liverpool these pipes have been successfully laid, but they have the provision of large sockets, and, in case of need, for lead joints to be made in them, which increased their cost. There is another method for using turned and bored pipes, termed a "combined joint," which is made by running lead into a wedge-shaped recess in the turned and bored joints, but it has never been extensively adopted.

Attendant on these systems there is a further cause of extensive leakage, which results from connecting mains with service pipes to supply the consumers. To join a service pipe with a large main, the workman finds the place of the buried main, and commences by drilling a hole of sufficient size for admission of a taper screw tap. When the hole is ready for the screwed end of a service pipe, the taper tap with its continuous thread has only imparted to the orifice some thread recesses, as the convexity of the main will not admit of full threads being made in the sectional area of the metal in small pipes, and therefore there are only some segments of threads made, into which the connecting pipe is screwed, which does but little more than stay it in the position required. As the pipe is always at right angles to the main, it is liable to the least strain from leverage, and, from its slight screw-hold at its junction with the main, sometimes even the filling-in and ramming of earth springs it at its junc-

tion, and bursts the holding threads. Such joints offer great facilities for escape of gas, and it is much to be doubted whether a service joint was ever made (except in a main of large size) that continued quite sound. Therefore, considering how many thousands of these connexions there are, and how these are in a continuous leaking state, it is not to be even doubted that 100,000 tons of coals are yearly necessary to supply the metropolitan gas leakage. There are some pipes made for the special purpose of making connexions, of which Mr. Cathels is the inventor. These pipes have a short strip of metal cast on them, and into this strip the hole is drilled and the connecting pipe screwed securely. But these pipes have the additional weight of metal which increases their cost, and it is only at the end where the strip is cast that such advantages are applicable. For making connexions these pipes will require the earth to be opened for a considerable distance, to find the available place on the main for making the connexion. Such may be a distance of five feet, whereas a two-feet hole is sufficient size for the workman, and this is considered too large by the contractor, who has to make the repairs good to the parish roads, so that these new pipes have this objection. A difference of opinion may arise as to the true causes of the loss of gas, but there cannot be any difference of opinion as to the fact that both the systems described cause great loss as well as inconvenience in their use, and that if a better method could be introduced it would be of no small advantage, provided the pipes could be manufactured at a price which gas companies are willing to pay, and that they do not possess the evils for which the systems now in use are condemned.

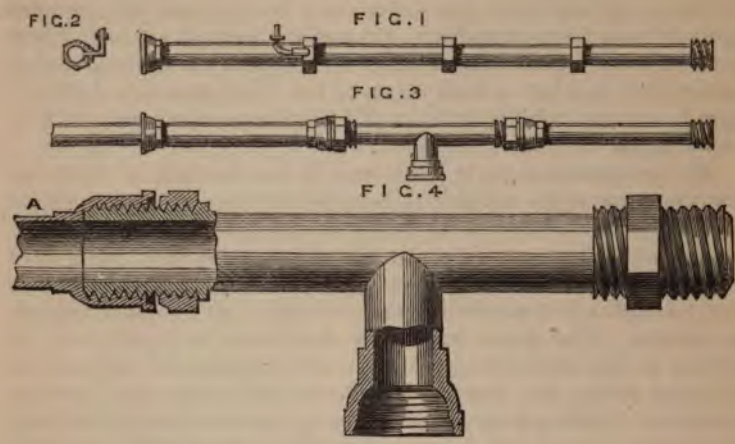
As a remedy for the evils which have been referred to, the author has designed a pipe possessing general effectiveness without additional weight or separate parts. It is entirely dependent on its form for keeping the joint with sealing materials to prevent leakage; it has a rigid joint, but one which will admit of altered levels, without prejudice to its utility. These advantages are obtained by making the pipes with screw and flange spigots cast with a male thread, and the socket with a female thread. By inserting the spigot into the socket, and causing the pipe to turn once round, the joint closes, compressing into a recessed channel at the root or end of each of the threads some moist cement, which is immoveably confined between the surfaces. At the same time the taper nose of the spigot enters the conical cavity at the root of the socket, in which is also a collar of moist cement. When the cement sets the joint is sealed, thereby adding strength and firmness to [illegible]oes for securing the joints. The thread is cast easily to [illegible]h a "lash," so that the screw is used for

a fastening grip only for joining the surface of the flange or lip of the spigot against the mouth of the socket. Any deviation of angle or direction requires but the temporary insertion of a nail or a piece of hoop-iron on the exterior radius to cause these pipes to sweep any ordinary curve that may be required, the inserted piece of iron being withdrawn prior to the cement setting. These pipes are capable of maintaining their position when any undue strain or concussion affects them at the joint, and are therefore free from that liability to leakage from those causes which affect other pipes.

By the rigidity of their joints and by the strength of their formation, these pipes are able to bear more than any usual amount of displacement, because their tensile strength becomes effective to resist it; also, by their continuity of joint, any superincumbent weight deflects not only a single pipe, but is transmitted through many lengths, so that each of the new pipes has a smaller amount to sustain. The pipes have been tried with a pressure up to 250 lb. to the inch without a weep of leakage, and are now in practical work; the cement is indestructible from any chemical operation of the gas upon it, and being immoveable in the parts holding it, reliance can be placed on the pipes, as the joints will remain as sound as when first tested. No disturbance of the joint can arise except the pipe be turned round to release it, and that is prevented by the next joint. The substitution of hexagonal for ordinary bands is a provision for adding strength to the pipe, also for tightening up the joint with a spanner, and for the prevention of leakage in making connexions for service pipes, without giving additional weight, and for allowing a number of connexions to be made if required on either side at four convenient distances, so that the excavations may not be unnecessarily large to find a fitting place for making a connexion. The flat surfaces of the hexagonal bands permit a continuous thread to be made of the full depth of metal in the pipe, and also leave a seating flat surface for locking and strengthening the joint of an inserted service pipe by a grummet washer and back nut. These joints are thus reliable for mains without the expensive adjuncts necessary in the lead and yarn joints of the common pipes, and they are also free from the expense of turning and boring, and consequently are a cheaper and more effective pipe than any now used.

This subject has been introduced in the hope that, if this new system does not possess such advantages as to leave nothing more to be desired, it will be useful in showing others what to improve and what to avoid. It is becoming an imperative necessity to have joints which do not leak, so that useless waste

may be avoided, that public buildings may be secure, and public health not impaired by poisonous exhalations, or life be lost by accidental ignition.



DESCRIPTION OF THE ENGRAVING.

Fig. 1 represents one length of the patent pipe, showing the hexagonal bands.

Fig. 2 is a cross section of the pipe with the service pipe attached.

Fig. 3 is three lengths of pipe, showing the T-piece by which a junction is effected with old mains.

Fig. 4 shows the T-piece on an enlarged scale with taper flange screw spigot, also showing lengthening screws and back nuts. The arrangement of the back nut for sealing joints on the lengthening screws is clearly seen at A, in this figure.

DISCUSSION.

The AUTHOR explained the method of manufacturing pipes upon his principle, referring to samples on the table.

Mr. ELLIOTT asked at what price the pipes could be delivered in London, and what was the size of the largest pipe that had been made?

Mr. BARKER said that the pipes could be delivered in London for 5*l.* 5s. per ton; the largest size that had been made and used was six inches; 8-inch pipes were, however, being made.

Mr. MORRIS said that Mr. Barker's pipe appeared better suited for gas-pipes than for water-mains. He questioned whe-

ther there was any room for the pipes to expand and contract without breaking the joints. When cast-iron pipes were first used they were made with flanged joints, and they had to be taken up because they were constantly leaking from the change of temperature. It appeared to him a good system of pipe and joint that the author had brought before them if the screw was made to fit nicely. He thought the leakage in bad joints of water-pipes was of no consequence, as the water was almost sure to rust the pipe a little and stop the leak. Of course in gas-pipes it was a different matter.

Mr. KERSHAW thought that these pipes being metal to metal would be rather too rigid to be laid, particularly in the streets of London, where it was difficult to get undisturbed ground to lay pipes in. The ground being made up, it settled unequally throughout the whole length of the main. Although the author said that the pipes were all settled together, still they could not insure that generally in towns, and these joints would be very apt to give way in the socket, which of itself did not look very strong. There was one method of jointing pipes adopted in London which the author had not described, and that was by making them of solid lead. They had been used very much in London for water-pipes; a small ring of lead was driven in first, and then the pipe was filled up in the usual way with melted lead. By that means a perfectly solid joint was obtained, and the iron surfaces not being in contact allowed for settlement. The bored and turned pipes had also been used in London. In Liverpool he thought they had used asphalte with a few turns of string put round and then driven home. He had used them with asphalte, but he thought it was too brittle, so a little tallow was mixed with the asphalte, and that made it more elastic. There was also red-lead and white-lead used round the joint. The red-lead made a very solid joint, but as the author said, there was some objection to that on account of the expense, because it involved the double joint.

Mr. GORE said that he considered the author's invention was a very close approximation to a system which had long been put in practice on the Continent. He considered the author's was too rigid a joint. If the joint was to be sufficiently tight to prevent leakage, it was then much too rigid to be used for the ordinary class of gas-mains, and that was the great objection to the use of turned and bored points. If the soil was of nearly equal density, and not subject to unequal subsidence, a tolerably perfect joint was obtained; but if one part of the main was laid in a soil that subsided readily, and another part in a denser soil, unequal subsidence followed, and the result was a drawing of the joint. That was what he apprehended must take place in con-

nexion with Mr. Barker's pipes also. There was another objection to this system, and that was, that the pipes could not be shifted or replaced in case of repairs. Another point was the effect of vibration on joints. In South America, immediately after the slightest shock of an earthquake, which exercised a vibratory influence upon the pipes, defects were invariably produced. If a pipe of the description before the meeting was placed in those circumstances, he could not see by what means the joint could be brought up taut again. With elastic substances they were easily drawn up by set and chisel. In practice he had not found quite such a list of objections to the ordinary methods of jointing as the author had stated.

Mr. ELLIOTT considered that the old-fashioned joint made with yarn and lead was objectionable, but he thought where the socket was reduced in depth, and in large mains where the lead was run solid into the socket and the joint set up nicely, a better joint could not be produced. The author claimed the advantage for his joint, because it could be made outside the trench. That was a thing which should not be permitted. Every pipe should be laid in the trench and jointed in position. Mr. Barker's joints were stated to be applicable to water as well as gas-pipes, but he (Mr. Elliott) thought a main 1000 ft. long, under a change of temperature of 30°, would contract 2¼ inches. In that case something must give way. The socket joint would allow for change of temperature: the author's certainly would not, and most probably the socket would give way. As to the cost of the turned and bored joints, he believed they had been offered to be delivered in London by the best makers at about 5s. per ton beyond the ordinary socket pipes. Then as to taking up and relaying; that did not often happen, perhaps once in ten years. The cost of the new pipes was stated to be 5*l.* 15s. a ton. A contract for about 4000 tons of pipes of various sizes had just been let for from 5*l.* 4s. to 5*l.* 8s., so he really could not see where the great advantage in the new pipes was.

Mr. LATHAM said that a similar joint to Mr. Barker's was brought under his notice some years since in earthenware water pipes. The only difference was the hexagonal castings in Mr. Barker's pipe, which he (Mr. Latham) did not consider would help to make a successful joint. As applied to water mains, one of its great drawbacks would be the want of expansion. Even with ordinary pipes after a severe winter breakages occurred, as well as leakages at the joints, which arose from the caulking material being drawn out by the movement of the pipes. For the past three or four years he had used the cold lead joint only, and was satisfied that it was thoroughly efficient. An 18-inch pipe jointed in that way, with a socket two inches and a half or three inches

in depth, and having a quarter of an inch of lead all round, required a much smaller amount of metal—about half the weight that the old-fashioned lead joint required. Besides, in the cold lead joint the cost of the yarn was saved. There was sometimes a difficulty in using molten lead owing to the presence of water in the pipes, and in some cases he had made a perfect joint with rings of cold lead driven into the socket and caulked up. The evils arising from the use of molten lead referred to by Mr. Barker were very rare in practice. The author had stated that it was necessary, in order to make his joint properly, that the screw should be perfectly concentric at both ends. When, however, these pipes came to be laid in a curve, he said, "put in a nail at one end and screw it up." By this means the very purpose of the joint would be defeated, for the screwed surfaces would not come properly together. A perfectly sound joint would therefore not be made, and failure would sooner or later result from its adoption. The joint would also fail from its rigidity, for when pipes were laid in the ground settlement could not be guarded against, and, therefore, if a settlement took place in a rigid system of pipes, the cast metal, incapable of bearing a tensile strain, would break, and in that way the system would fail. The difficulty of laying the pipes up hill and down hill, to which the author had referred, was purely imaginary, and was one he had never met with in his practice, or had even heard of before.

Mr. Fothergill said there was one very important point in connexion with these joints, and that was when a number of pipes were put together, forming a whole; if any particular pipe required to be disengaged, there was no room endways for disjointing the one from the other; whereas in the ordinary system of laying pipes by taking out the packing, the pipe could be liberated and a little side play obtained by which the object could be accomplished.

Mr. Alfred Williams asked the author of the paper what description of cement he used in the joints. The author told them that the pipes laid in the usual way with lead and yarn, through the deflections and vibration of the ground, got out of their level; but he had brought before them a pipe in which he said the screw did not make the joint, but that it was made with cement.

Mr. Barker said that the joint was made with Portland cement.

Mr. Williams, continuing, disagreed entirely with the use of cement for such a purpose. The author had complained about workmen laying the old pipes carelessly, and in consequence making bad joints, causing leakage; but there was no-

thing to insure the author's joint from being laid carelessly also. The author also complained of the pipes being put together in the road, and consequently made concave or convex, according to the road, and then rolled into the trench, and thus the joints got broken. He (Mr. Williams) had always seen the pipes made up on a cradle, although he objected to that method of laying pipes. They should be laid in the trench in a proper position and properly packed up. He thought the author was in error when he stated that with Mr. Cathels' pipe it was found necessary, here and there, to open a five-feet length to find the spot where the service was to be put in. If he (Mr. Williams) recollected rightly, Mr. Cathels' pipe had a strip from end to end about two inches wide running along the top or side on both sides. He had laid a great many pipes, and had had them tested night by night with a fire at every joint; and he had no occasion to touch one of these joints afterwards, nor had he after two or three years ever found a leak from mains laid with proper care. The question of leakage was a moot point connected with gas manufacture, and the manager was often wrongly blamed for leakage. He (Mr. Williams) believed that a very large proportion of what was called leakage was condensation, and not leakage at all.

Mr. GORE said that some years ago he made experiments to ascertain the cause of leakage, and also with regard to the revived notion of the endosmose and exosmose action which was going on in pipes. He used a piece of pipe 400 yards long, and tested it with water, with pressure gauges, and every other test available for the purpose. By constant attention and painting, the leakage from exosmose or endosmose action, and leakage of joints, was reduced to four cubic feet per hour, as measured by a meter. Taking the quantity of gas made, the temperature at which it passed through the meter, and then the temperature at which it issued from the gas holder and entered the main, and the quantity that was delivered at the other end of the main through the meter, it gave a leakage upon that piece of main of $14\frac{8}{10}$ per cent. That proved to him most conclusively that the street leakage, of which so much had been said, was far more imaginary than real. The reduction in the temperature of the gas was the cause of the apparent loss, the reduction existing really in the bulk.

Mr. WRIGHT asked the author how he proposed to put in or take out a length of pipe with his joint?

Mr. MARDOCK asked what width of trench would be required to lay a 24-inch main of Mr. Barker's pattern? He (Mr. Mar- considered that large mains of that kind would prove very d to lay in London thoroughfares. As to making lead nd rolling the pipes on an uneven surface after they had

been caulked, that was scarcely ever done in London. Every length of pipe was jointed in the trench. In the course of three or four years he (Mr. Mardock) had seen three or four hundred services put in, and the ground excavated down to the mains, and only very rarely did he observe any signs of leakage. In his opinion the lead joint, properly made, was a better joint than Mr. Barker's.

Mr. COLYER said he thought it would be almost impossible to cast pipes like Mr. Barker's with screws that would be sufficiently true to be of any practical value. In the next place they would be too rigid, and would not allow for any undulation in the ground, and certainly not for much expansion. He had used socket pipes with lead joints, both for gas and water; he had also tried them for steam, and had never found such difficulties in laying them as the author had described; and when the joints were properly made, he had never known them to fail. He would mention a pipe he had seen used for steam purposes. It was a flange pipe, or rather a pipe cast with two or three clips upon it according to its size, and which had what he thought was called a thimble joint. It was better known in the north than in London. The pipes were bored out a little coniform, and had a copper thimble inserted between the two bored sockets, and drawn up by bolts. For steam purposes, they were the best things he had seen. They were chiefly used in sugar-houses; and a joint that would stand sugar-liquor, would stand anything.

Mr. OLRICK observed that it had been stated that the loss of gas did not occur so much from leakage as from condensation. If there was not leakage, how was the blackened condition of the soil, which presented itself whenever the streets of London were opened up where there were gas pipes, to be explained? He had been told that there was not only leakage in the joints, but that there was also a leakage through the cast iron, of which he had no doubt, and it was due to the badness of the cast iron, the gas companies not paying a price which would secure good pipes. He wished to know whether Mr. Barker used the same cement for gas as for water-pipe joints?

Mr. LATHAM said he thought there would be great difficulty in manufacturing Mr. Barker's pipes on a large scale, as pipes of large size, to be perfectly sound, should be cast perpendicularly; and, therefore, as the spigot end of the pipe was always to a certain extent honeycombed, it was always necessary that a portion of that end should be cut off. Therefore, with Mr. Barker's pipe the best plan for casting large pipes could not be resorted to, for if an uncertain length of pipe had to be cut off, pipes of the character under notice would be destroyed.

Mr. BARKER, in reply, said that the question which appeared to him to be necessary to answer first, was, how a length of his pipes could be taken out and replaced, if required. There would be one length of pipe with a socket at each end, and one would have an extra length of thread upon it, so that it would be screwed in, and then would be drawn back and inserted in the length at the other end, and the nuts would lock it up with the cement upon the surfaces. There was more strength in his hexagonal nut and band for making connexions than there was in ordinary pipes, as the flat surface allowed a longer thread to be cut, and consequently a better hold to be obtained, than in a pipe where both surfaces were curved. There was also additional strength given to the inserted connecting pipe by the back nut having its seating upon the flat hexagonal band. With regard to Mr. Cathels' pipe, he had not seen any invention in which Mr. Cathels carried a strip from end to end. The patent claimed two lengths of band put upon the pipe, one of which was put up to the socket (that was not put upon the whole length); it was longitudinally placed, but went off a short distance—he believed one-fourth the length. He had been asked the width of trenching necessary for a 24-inch pipe; but as he had never had one of that size made, he could not answer the question. It had been said that his pipes had not the advantage which he believed them to possess, as they required a wedge or nail inserted to give them curvature in laying them. He had stated in the paper that the threads were made with a lash, so that they did not fit like a turned and bored socket joint, but they worked well about, and by screwing it up and this lash with it, and temporarily inserting a nail or wedge in one portion of it, the pipe could be led over to one side, and in a nine-feet length considerable divergence could be given, then; by withdrawing the nail the cement set, sealing the joint. He had found by actual fact, that when he wanted to go in a different line, so that he might get a slight curve in laying these pipes one within the other, all he had to do whilst the cement was wet was to place a small nail temporarily in the point which the pipe did not bite upon, and that would hold it for a time until the cement had set. The action of this pipe was not so much that of a screw pipe, but its principle was more that it held within its cavities cement which when set was immovable, and could not be displaced. As to the leaking, anything which leaked from it must come out through the cement before it could get out of the pipe, and that he found required a pressure far greater than gas 'l give, and therefore he believed that the joint would not

Most of the remarks which had been made during the ıg as to the instability of his pipes, had reference to water, gh he had not alluded to water in his paper, as his pipes

were intended chiefly for gas. As to expansion and contraction, the pipes had no means of obviating that, further than the longitudinal strain which they would take. The cement that had been used for the joints was Portland cement, and as they had been found to bear all the tests that had been applied to them, up to 200 lb. on the inch, they were sure to prevent the evil of leaking at gas main joints. His was a joint for that, and for no other purpose. With reference to the laying of pipes on the side of a hill, he knew of a case where all the main joints in a town had to be remade, in consequence of the weight being brought upon the lowest pipe which bore the greatest amount of strain. Another case occurred in Buckinghamshire, at a mansion where the pipes were laid up a very steep hill, and they were similarly affected. The lead was drawn from almost every joint. An allusion had been made to his socket, which it was said was not sufficiently strong. He considered that there was a greater strength in his socket than there was in those of other pipes used. With regard to cost, the price he had mentioned was that at which they were first manufactured, and they were made under all the difficulties incidental to the first manufacture. They could, however, now be made at a much less cost. As compared with the lead joint, those he had described were certainly much cheaper in the putting together, and saving lead and yarn; and as compared with the turned and bored joint, they saved the expense of turning the pipe. If the old lead joints did not leak, how could eminent engineers make such statements before the Parliamentary Committee, where the whole subject had been thoroughly investigated, and where it was very well understood that one-sixth of the gas made had been lost through leakage, as Mr. Clegg had stated in his evidence.

The PRESIDENT said that he thought he might take it that the opinion of the meeting was that the joint described by the author of the paper was too rigid, and that the present lead joint was preferable. The discussion had elicited a great deal of information from many practical men, and they therefore had to thank the author on that account, as well as for the trouble he had taken in preparing his paper.

April 5th, 1869.

F. W. BRYANT, PRESIDENT, IN THE CHAIR.

THE METHODS EMPLOYED IN THE DETERMINATION OF THE COMMERCIAL VALUE AND THE PURITY OF COAL GAS.

BY F. W. HARTLEY.

IN the purchase of most of the articles of ordinary requirement, the purchaser is able, to a very considerable extent, to exercise judgment as to the quality and consequent value of the articles in question. It is true that a purchaser may be, and often is deceived, however careful he may be; but still he must, perforce, rely solely on his prudence and judgment to secure due value for his money when he purchases, inasmuch as the law provides him with no special means for his protection. In large transactions in articles which are variable in quality and which are capable of analysis, the vender or buyer frequently obtains the assistance of a skilful chemist, and the quality or purity, and therefore the commercial value of the article, is determined by analysis. With respect to coal or other illuminating gas, it is scarcely possible for the purchaser to determine by the exercise of judgment whether that with which he is supplied is of the quality he is entitled to receive. Over the quantity he has a power of control by regulating the supply with governors or with the main cock, and by the use of a meter, which has been verified as to correctness of measurement by a public inspector. But over quality, he can exercise no proper supervision. It is true that he might engage a skilled operator from time to time; but this would be expensive; or he might have testing apparatus of his own, learn their use, and employ them in his house; but i[illegible] as testing apparatus cost a good deal of money, require [illegible]t apart for them, much experience and a good deal of

skill in their manipulation, it is manifest that this latter course could not be generally adopted. Moreover, it would be most imprudent to give to non-official operators the power of seeking for penalties, or for compensation from gas companies on account of deficiency, or alleged deficiency in quality.

Seeing that the purchasers of gas are not, and cannot be, generally put into a position to protect their own interests in this matter, agreements have been made between the gas company and the authorities, on behalf of the public, in nearly all the large towns and many of the smaller places in the United Kingdom—agreements which have been inserted as clauses in many Acts of Parliament—that the gas should be of a certain illuminating quality and degree of purity, as certified by a public officer duly appointed to test the gas. In case of failure to maintain the gas at the specified quality, the companies were rendered liable to penalties recoverable before a magistrate on the evidence of the testing officer. In some cases, it is stipulated that the testings shall take place daily. In other places, the times of testing are left to the discretion of the testing officer. These agreements, when carried into practical effect, undoubtedly protect the gas consumer; but, unfortunately, no attempt was made until a recent date to define the precise manner in which the testings should be made—a defect, prejudicial both to the public and the gas companies. The amplest specification was, that common coal gas should be burned at the rate of five cubic feet per hour from a 15-hole argand burner with a 7-inch chimney, and give a light equal to so many sperm candles, of six to the pound, each burning at the rate of 120 grains of sperm per hour.

In 1860, a distinction was made in the burners to be used for cannel coal gas, but simply to the extent of specifying that they should be fish-tail or bat-wing burners, the five-feet per hour consumption being still adhered to. In respect to purity, the gas was to be so far free from ammonia as not to discolour a turmeric test-paper exposed for one minute to a current of gas issuing from an orifice at $\frac{5}{10}$ of an inch water pressure; while in respect to sulphur compounds, the gas was to be free from sulphuretted hydrogen, and not contain more than 20 grains of sulphur in any form per 100 cubic feet. Now as, in the first place, the amount of light obtainable from the combustion of a given volume of gas depends, like the power obtainable from the explosion of a given weight of gunpowder, upon the conditions under which it burns, and as there is almost as much difference between burners nominally of the same description, as there is between an old-fashioned, ill-made smooth bore and a rifle of the best

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construction, it is evident that, in the face of so loose a specification, the burners would induce differences in the results of various operators in respect to the same gas, and lead to disputes as to its quality. In the next place, no particular form of photometer was specified. This omission has led to little or no evil, owing to the fact that the Bunsen photometer has been employed, and generally of the same form, or very nearly so. But the omission might have caused differences and disputes, if any operators had chosen to use any other and less perfect photometer. Lastly, in respect to the testing for sulphur, no mode was detailed as to how this was to be done; and in spite of the efforts which have been made to secure uniformity in practice, very considerable improvements need still to be made in the apparatus, and all the details of the *modus operandi*, to the minutest point, agreed upon, before uniformity of result can be secured even by the same operator.

There can be no question that the conditions imposed as to the freedom of gas from ammonia and sulphur were too stringent, and that they were agreed to in the absence of a sufficient knowledge. In the most recent example of legislation on the subject of gas-testing, which took place on the passing of the City of London Gas Act last year, Parliament wisely determined to authorise the Board of Trade to appoint three competent persons, to form a sort of permanent commission, to be styled gas referees, whose business it should be to define, from time to time, the standard of purity of the gas, determine the methods by which it should be tested, and issue precise instructions for the guidance of the actual operators, who are called gas examiners. The Board of Trade, some months since, appointed Messrs. J. S. Pierce, F. J. Evans, and R. H. Patterson, as the gas referees, and their first instructions were issued to the gas examiners in January last. These instructions will be referred to in the course of the paper.

The commercial value and purity of gas depends, as already stated, upon—

Its illuminating power;

Its freedom, to a certain extent, from ammonia;

Its freedom from sulphuretted hydrogen; and

Its freedom, to a certain extent, from sulphur in any form.

As previously remarked, the amount of light obtainable from the combustion of gas depends very much upon the manner in which it is burned. It appears from documentary evidence that, in the v[illegible]rly days of gas lighting, the construction of burners was [illegible]dered, and the conditions necessary for the produc[illegible] best effect thoroughly understood; but although

those conditions have been again and again laid down by competent men, so little regard has been paid to their teachings, that not only have burners in almost infinite variety been made during many years upon false principles, but it is even supposed by many scientific and practical men, that our present knowledge about the construction of gas burners is due almost entirely to the investigations of recent experimenters.

So far back as forty-three years ago, Christison and Turner published a statement of their investigations, together with certain conclusions, which the author of this paper has summarised as follows:—

1. That up to a certain maximum consumption for each burner, the light increases in a much greater ratio than the consumption of gas.

2. That for each burner there is a certain size of flame which is most economical: this is but a corrollary to the first proposition.

3. That in argand burners, the *size* of the holes and their *distance from* each other is of the highest importance. The holes should be so near to each other that the flame unites at its base. For gas of specific gravity 550 to 650, the holes should be $\frac{1}{32}$ in. diameter, and $\frac{12}{100}$ of an inch apart. For gas of a higher gravity, the holes should be $\frac{1}{50}$ of an inch diameter.

4. That the size of the central aperture of an argand exercises an important influence on the amount of light yielded.

5. That the greatest amount of light is obtained when the flame becomes tinged with yellow, and is near to the point of smoking.

6. That the glass chimney should be proportioned to the size of the burner and the consumption desired.

7. That consumers generally cannot burn the gas in such manner as to produce the best effect, on account of the liability of the flame to smoke.

These propositions really comprise all that is know respecting the principles which should govern the construction of gas burners, and recent experimenters have done no more than demonstrate the correctness of the above propositions, and possibly to put them into more precise and definite expression. In respect to the sixth proposition, practical objections, which the propounders themselves saw, exist to its application; which are, that if glass chimneys be reduced to very small diameters, the glass is apt to become partly fused and opaque; the chimneys are liable to frequent breakage, and further, flames inclosed in very narrow chimneys are extremely liable to smoke when the air currents are at all disturbed by draughts or other influences.

Clegg and Peckston were among the early teachers on the subject of gas burners, other teachers followed, among whom may be notably mentioned, Alexander Wright, Lewis Thompson, Dr. Letheby, and Henry Bannister. The late Alexander Wright, some fifteen years or more ago, expressed the result of his observations lucidly and shortly as follows: "An argand burner, properly constructed, will give more light than a bat-wing, and a bat-wing more light than a fish-tail burner, each consuming the same quantity and being equally adapted for the gas. The more gas each burner consumes, the more light will it give per cubic foot. Five cubic feet of gas will give more than double the light obtainable from $2\frac{1}{2}$ cubic feet burned in the same time from a smaller but similar burner. The best result is obtained when the burner gives a regularly formed flagging and rather dusky flame, and the worst result is obtained when it gives a wire-drawn and irregular flame."

Lewis Thompson in the "Chemistry of Gas Lighting," published in 1851-2, says: "Every burner has a certain fixed amount of each quality of gas which it will consume to the greatest advantage. Every form of burner acts with its maximum effect, when its flame is on the verge of smoking. The quantity of light is greatest with the argand, and the intensity with the fish-tail. (The difference he ascribes to the greater contact of atmospheric oxygen with the fish-tail.) Poor or common coal gas should issue more gently, that is, at a lower pressure than rich or cannel gas, and from burners having larger holes than those for the latter gas."

The yellow-tinged flame, the flagging flame, and the gentle current, all mean the same thing, viz., low pressure, and MM. Dumas, Reynault, Andouin and Berard, have established as a general law, that the greatest illuminating power is obtained with low pressures, and the maximum light with pressures equal to ·079 to ·12 of an inch head of water. They state further in respect to various burners:—That bat-wing burners of the same diameter and burning the same quantity of gas, produce a greater quantity of light when the slits are increased in width, $\frac{1}{36}$ of an inch width, producing in their experiments the maximum effect. The diameter of the burner should be proportioned to rate of consumption desired, but is less important than the width of slit. That single jet burners are very disadvantageous for light-giving purposes. The influence of the diameter of the holes is much less apparent than in bat-wing burners. That no great advantage is yielded by the fish-tail over two single jets, with holes of the same diameter, if the holes be very small. Two single jets $\frac{1}{36}$ inch diameter will give the same light

as a fish-tail with holes of the same size. For consumptions ranging from $3\frac{1}{2}$ to $5\frac{1}{4}$ cubic feet per hour, holes $\frac{1}{17}$ to $\frac{1}{18}$ of an inch diameter are the best, and for these consumptions the union jet or fish-tail is inferior to the bat-wing with large slits. That argand burners of almost the same appearance may require to burn double the quantity of gas to give the same quantity of light, and this is dependent:

1. On the diameter of the holes or the width of the slit.
2. On the number of holes.
3. On the manner in which the air is distributed, which is dependent on the actual and relative dimensions of the apertures by which the air gains access to the interior, and to the exterior parts of the flame.
4. On the height of the chimney.

With respect to the first three conditions, their conclusions are in absolute agreement with those of Christison and Turner. With respect to the fourth, they state that an increase in the length of the chimney increases the consumption for the same amount of light, the difference between an 8-inch and a 10-inch chimney being from 5 to 7 per cent. in favour of the 8-inch one. The foregoing conclusions relate to common coal gas, but the principles remain precisely the same for the richer gases, with the single exception that the burners for the latter must be made to consume the gas under somewhat higher pressures, so as to secure thinner flames, and more surface contact with the atmospheric oxygen.

The investigations of Dumas and associates were so exhaustive, and those gentlemen expressed the principles which should govern the combustion of gas for lighting purposes so definitely, that a great impression was created in the minds of scientific and practical men, and Dumas and associates are frequently credited with having propounded and established principles which they only, in fact, confirmed. The author has, therefore, taken the opportunity while treating of burners to claim credit for some of his countrymen, who at much earlier periods propounded the same principles.

Down to the year 1860, so great a difference existed in the burners employed in the determination of the illuminating power of gas, that the light evolved by the combustion of gas of the same quality at the parliamentary rate of five cubic feet per hour may have differed at times to the extent of 20 per cent. Each operator was at liberty to choose the burner which he fancied to be the most suitable, and, as a necessary consequence, controversies arose as to the actual quality of the gas. In the year above mentioned, Mr. George Lowe presented to a Committee

of the House of Lords on the Metropolis Gas Supply, an argand burner, which, whether intentionally constructed, with a full knowledge of, and due regard to, correct principles, or whether produced by accident, for the evidence on this point is exceedingly contradictory, burned the gas under much more favourable conditions as to pressure and air supply than most of the burners then in use. No specification of the dimensions of this burner was given in the Act of Parliament of 1860, but the burner came into very general use after that year.

The author has selected a series of argands, which illustrate the importance of the size of the jet holes, central aperture, &c. The chimneys are all of the same size, viz., $1\frac{7}{8}$ in. diameter and 7 in. high, but the dimensions of the burners vary, as shown in the following table, which gives also the proportionate light evolved by each, when burning common coal gas at the rate of 5 cubic feet per hour. No. 1 has a steatite top; the others have iron tops.

	No. 1.	No. 2.	No. 3.	No. 4.
	Inches.	Inches.	Inches.	Inches.
Size of jet holes	·06	·05	·055	·03
External diameter	1·1	1·1	1·108	1·08
Diameter central aperture	·45	·49	·52	·54
Pressure below burner	·045	·05	·05	·18
Light given	100	·94	80·5	74·3

With respect to flat flame burners, which are generally used in determining the illuminating power of cannel coal gas, the Act of 1860 is also, unfortunately, not specific. The external dimensions, the width of slit, or size of the jet holes, of the burners, and the pressure at which they were to burn the gas at the required rate, should have been stated, and also whether the side or the edge of the flame was to be presented to the photometer, for although it is undoubtedly true, that the number of rays emitted from a mere point of light are equal in all directions, it has been found that a flat flame emits less light from the edge than it does from the side, owing, no doubt, to part of the flame being further removed from the object illuminated, and to the want of absolute transparency in artificial lights. In practice, however, the companies are fairly dealt with by the presentation of the side of the flame to the photometer. Any of the bat-wings in the following table might, under the Act of 1860, be chosen for use in testing cannel gas, although they prod[illegible]ry different effects.

	No. 7.	No. 8.	No. 9.	No. 12.
	Inches.	Inches.	Inches.	Inches.
Diameter of the burner . .	·304	·31	·324	·344
Width „ slit	·013	·013	·017	·017
Depth „ „	·14	·16	·18	·23
Pressure below burner . . .	·675	·47	·3	·25
Light given for 5 cubic feet per hour	77·5	92·7	98·4	100

As already stated, the standard light in this country is that produced by a sperm candle, of the size of six to the pound, burning at the rate of 120 grains of sperm per hour. This standard is without doubt defective. It is difficult, if not impossible, to obtain candles of uniform quality, both in respect to the sperm and to the wick, and in addition age has also an effect upon their rate of burning. It is rarely that a candle is found which burns at so low a rate as 120 grains per hour, and hence a true standard light is seldom obtained. It is usual, however, to reject candles which diverge very widely from the standard rate, that is, to an extent of more than about ten per cent., for there are good reasons for believing that with candles as with gas burners, the variation in the light given is, under favourable conditions, greater proportionately than the variation in the rate of consumption. No sufficient data has been established upon which to found a rule, and therefore it is assumed in practice that the light emitted by the candle is in direct proportion to the rate of burning. Thus the light of a candle burning at the rate of 120 grains being taken as equal to one, the light for 130 grains would be taken as equal to 1·0833.

In France, the standard light is produced by the combustion of colza oil burned nominally at the rate of 42 grammes or 648 grains per hour, in a carcel lamp, the size and the height to which the wick is to be raised, and the dimensions of and conditions under which the chimney-glass is to be used being all precisely defined. This standard is, in the author's opinion, even worse than the candle, for there is no uniformity in the rate of combustion under apparently identical conditions, and the variation in the light is very great. The author made, some time ago, some hundreds of experiments with carcel lamps, which, with wicks, chimneys, and oil, were obtained direct from the firm in Paris who supply the testing-houses in that city. The results of these experiments were very adverse to the lamp.

Various schemes for the production of a standard light have from time to time been proposed. It would occupy too much time to enumerate all these schemes, but one, the most promis-

ing of all, which has been proposed by Mr. Keats, must be mentioned. That gentleman proposes to use a specially constructed lamp, under specific conditions, burning sperm oil at the rate of 750 grains per hour. With this lamp a close approximation to regularity of consumption of light is secured, and the value of the light obtained is equal to about ten sperm candles. The lamp, and the balance which Mr. Keats has constructed for use with it, are exhibited on the table, and details of experiments are published in the Journal of Gas Lighting of March 16th, 1869.

The relative illuminating power of light is determined by means of the instrument called a photometer. A photometer properly so called is an instrument which is based upon: first, the power which the eyes possess of judging of the equal illumination of different parts of the same object; and, second, on the laws which govern the diffusion of light. Various kinds of photometers have from time to time been employed, but now there are but two, which are much used, namely, Foucault's and Bunsen's. The first is used in France, and is a modification of Romford's shadow photometer. The object to be illuminated consists of a semi-transparent screen, from the centre of the back surface of which and at right angles to it projects a thin opaque diaphram, some two or three inches wide. The screen and diaphram form in section a T-shaped figure. The lights to be contrasted are placed at a little distance apart on a rigid bench, or table, and the diaphram side of the screen is presented to a point midway between the lights, so that lines drawn from corresponding points in each half of the screen to the nearest light respectively shall form equal angles. The lights cause the diaphram to cast shadows on each half of the screen, but if the lights be equal all appearance of shadow on each side is destroyed. If the lights be unequal the weakest may be moved nearer the screen, or the strongest removed farther away. In France, the gas flame is required to give a light equal to that of the carcel lamp, for an expenditure of not more than a certain quantity of gas, therefore equality is obtained by increasing or diminishing the gas supply.

In this country, Bunsen's screen is employed, and was first applied here to photometry by the late Mr. Alfred King. It consists of a piece of white paper, 3½ in. or 4 in. diameter, impregnated, with the exception of a spot in its centre of about 1 in. or 1¼ in. in diameter, with wax or sperm. The paper employed is as free as possible from water-marks, or wooliness. Such a screen is of unequal translucency, and when held between the eye and a light, the centre spot appears comparatively dark, but if the

screen be illuminated equally on each side, its translucency will disappear, as will also the greater opacity of the centre spot, owing to the reflected and transmitted rays being balanced in their effect. The disappearance of the centre spot is, however, true only in theory, for the texture of the paper necessarily differs in the greased and ungreased portions, and this difference is nearly always manifest: in addition to which, the lights contrasted are seldom of the same colour, and the greater this difference is, the less likely is the spot to disappear. Nevertheless, sufficient neutralisation of translucency is produced to enable the experienced operator to decide with ease and a considerable degree of certainty when equality of illumination exists.

It is a well-known law that the intensity with which an object is illuminated is in an inverse proportion to the square of the distance of the object from the light, or source of light. If a light be put successively in the centres of globes of 2 ft., 4 ft., and 6 ft. diameter respectively, it will in each case illuminate the whole interior surface, but inasmuch as the superfices of globes are to each other as the squares of their radii, it follows, that while the quantity of light thrown upon a given area, say one square inch, would, with the 2 ft. globe, be equal to one, it would with the 4 ft. globe be equal only to ·25, and with the 6 ft. globe be equal only to ·111. If it be conceived that the screen, when equally illuminated by two flames, forms part of the boundary of two globes, in the centre of each of which one of the lights is situated, then the number of superficial inches in each globe will express the relative quantity of light given by the flames. These considerations are usually expressed by the law that lights which equally illuminate an object are to each other as the squares of their distance from the object.

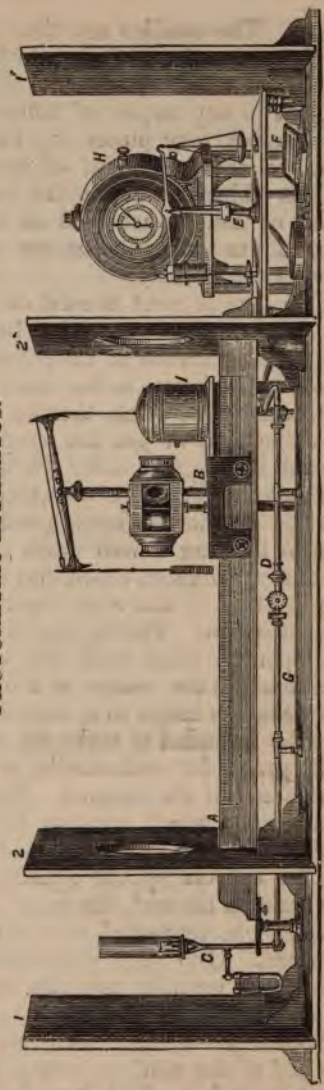
The Bunsen photometer is made in somewhat different forms, and ranges in length from 50 in. to 100 in. between the lights. That exhibited, and which is illustrated at Fig. 1, is Dr. Letheby's form, and this, in the author's opinion, is the best Bunsen photometer extant. It consists, like most Bunsen photometers, of a wood rod, about 4 in. broad, set on edge; the length in this kind being 60 in. between the lights. It, like other photometer rods, is graduated into squares of distances, which represent the value of the stronger light in multiples and decimal parts of multiples of the value of the weaker light. In common with most other Bunsen photometers, a slide or saddle fits upon and carries the Bunsen screen at right angles to the rod. In this photometer the screen is inclosed in a small box which is open at the ends and in front, and which is provided

with two reflectors set at an angle, so as to reflect the two sides of the screen to the eyes. The instrument is further provided with four rectangular blackened screens, which prevent to a great extent the diffusion of light into the operating room, and cut off from the operator's eyes all rays save those reflected from the screen. With ordinary photometers it is usual, and indeed necessary, to blacken the ceiling and walls of the operating room, in order to prevent the interference of reflected light, but with this instrument such a precaution is hardly needful, especially when the photometer is provided with black curtains in a manner similar to that exhibited.

A test of the illuminating value of the gas is made by comparing the light of a flame produced by burning the gas at the rate of five cubic feet per hour with the light produced by the flame of one candle, or with the flames of two candles, made of sperm, and six of which weigh one pound. In order to determine the rates of consumption it is necessary to employ a balance to weigh the candle or candles, an accurate meter to measure the gas, a governor to maintain regularity in the gas supply, a cock with a micrometer attached to its plug for making minute adjustments in the gas supply, and finally, a thoroughly well-made clock to measure time. The balance is usually separate from the photometer, and the candle or candles have to be removed in order to be weighed. The author has effected an improvement by combining the balance with the photometer, so that the candles can be weighed while in actual position for use, and without the necessity for extinguishing them. It is but fair to observe that Mr. Keats was the first to publish a method of weighing the candle while in position for use, but his system was different from that of the author, and required a specially constructed balance, which was scarcely available for other uses.

The index of the meter is constructed to show in one minute the rate of consumption of gas per hour. The governor is a delicate automatic instrument, which, when adjusted to give a specific pressure of gas at its outlet, will maintain that pressure uniform, notwithstanding that the pressure of the gas entering it may vary considerably. It is of course to be understood that the governor is only a regulator of pressure, and that the gas must always enter it at a higher pressure than it is required to be discharged at. The micrometer is interposed between the governor and the burner. The minute clock is capable of indicating seconds and of striking each minute.

The following is a description of the apparatus, reference being made to [illegible] of the engraving:



PHOTOMETRIC APPARATUS.

FIG. 1.

A. Photometer bar graduated into squares of the distances between the lights.

B. Slide supporting the box which contains the disc and reflectors.

C. Gas-burner on upright gas-tube fitted with pressure gauge.

D. Horizontal gas-tube connected to C, and fitted with micrometer cock at D.

E. Balance, carrying-holder for two candles and counterpoise pan.

F. Box of weights, beside which is a circular box for shot, used in counterpoising the candles.

G. Base board to which the apparatus is fixed.

H. Experimental meter on stand.

I. Experimental governor.

1, 1′, 2, 2′. Screens with dull black surfaces, to prevent the interference of reflected light and to shield the operator's eyes from the direct rays of light from the flames.

The gas first enters the meter, by which it is measured, passes next into the governor, and thence through the tubes D and C to the burner.

The proceedings necessary for an experiment are as follows: Several sperm candles are cut into halves, and the wicks of the lower halves exposed by cutting away a portion of the sperm, the halves are all lighted and are allowed to burn in a place free from draughts until a well-formed wick of maximum size is in each case attained. The candles are then all carefully extinguished by touching the top of each wick with a piece of sperm, and immediately blowing out the

flame. The candles are then ready for use. If with any one of the pieces the wick is out of the centre of the candle, or the candle will not form a tolerably regular cup, or if, in short, it appears in any respect of inferior quality, it is at once rejected. Two satisfactory pieces are taken, preferably the upper half of one, and the lower half of another candle, and placed in the sockets which are suspended from the end of the balance. Two pieces of candles are used to secure, in the mean, the nearest approach to the parliamentary rate of burning, and pieces of two candles may possibly give the nearest approximation.

The gas is next turned on to the burner and lighted, the micrometer cock being first fully opened, the governor is then adjusted by means of its pressure weights until the hand of the meter index makes rather more than one revolution per minute, as timed by the clock, thereby showing a consumption of about 5½ cubic feet of gas per hour. Next the micrometer cock is turned by degrees partly off, until it is seen that the pointer of the meter index makes an exact revolution in each minute. The candles are now lighted, great care being taken on every occasion of lighting to well warm the sperm at the base of the wick with, say, a lighted taper, and thereby prevent the combustion of any portion of the wick before it can obtain a full supply of melted sperm. The candles are then nearly counterpoised, and the operator waits until the balance turns, and its pointer or cock touches the centre of the attached arc; the time at which this occurs is taken to a second by the aid of the minute clock. If it be intended to make ten minute weighings, a weight equal to 20 grains for each candle, or 40 grains in all, is placed on the candle-end of the balance. Then comes the actual photometric experiment, which consists in sliding the saddle carrying the screen along the rod, until the two images of the screen reflected by the mirrors appear equally illuminated. By reference to the scale on the rod, the value of the gas flame for illuminating purposes, as compared with the value of two sperm candles, is obtained. One such observation is made for each of the ten minutes, if possible, and the mean of the whole is taken as the value of the gas.

Should the operator have no assistant, he devotes himself at the end of the ninth minute to the balance, in order to catch the exact second at which it again turns. Should he miss seeing it turn, he has but to add another 40 grains to the candle side, make fu[illegible] comparisons between the lights, and be more careful [illegible] in good time to the balance. Under these cir-cumst[illegible] mean of the photometric readings for twenty mins[illegible] taken.

[illegible], but not so good a one, of finding the con-

sumption of the candles, is to weigh them before lighting, and then to light them with great promptitude when the clock strikes or indicates the commencement of a minute. When, say, ten minutes have expired, the candles are to be extinguished with equal promptitude and re-weighed; the bright tops of the wicks being touched with sperm to prevent smouldering. If it be supposed that the mean of the photometric observations gives a light 6·4 times greater than that produced by two candles, or the light of 12·8 candles, and that the candles burned 40 grains in 9 minutes and 40 seconds, or in 580 seconds—the time due to such consumption being 600 seconds—it would be assumed that the candles gave more than the standard light, and the value of the gas would be corrected by proportion, and its value raised to 13·24 candles—

$$580 : 600 :: 12{\cdot}8 : 13{\cdot}24.$$

If the candles exceed the time due to the consumption of a specific quantity of sperm, a similar correction would be made, taking the proper time (600 seconds for 40 grains with two candles) as a constant multiplier of the observed value. By the second method of weighing the number of grains burned in a given number of minutes is determined, and the number of grains, instead of the seconds of time, is used in correcting for the consumption of the candles. Supposing that the relative value of the gas flame were, as before, 12·8, and that the candles burned 41·38 grains in ten minutes, then the corrected value of the gas would be 13·24 as before—

$$40 : 41{\cdot}38 :: 12{\cdot}8 : 13{\cdot}24.$$

The gas referees lay down a condition that no photometric experiment is to be recorded in the public testings in London, in which the candles vary more than 5 grains each per hour from the standard quantity. This would limit the times for the consumption of 40 grains in the first system of weighing to between 576 and 626 seconds, and the consumption for ten minutes in the second system between 41·66 and 33·33 grains, two candles being in each case employed. The author's experience leads him to doubt whether sperm candles can be regularly produced which will meet the conditions imposed by the referees.

It has not been usual in testing gas on behalf of the public, or indeed generally, to correct the volume of the gas for temperature and barometric pressure, but the referees have directions to be made in the London testings, and hence the volume of the gas, as measured by the meters, is to be reduced to a standard volume at 60° Fah. and 30 in. bar. Five cubic feet of gas measured by the meter when the thermometer is at 56 deg., and the barometer is at 30·5 in., would, at 60° and 30 in.,

measure 5·125 cubic feet. The density of the gas would be greatest at 56°, and 30 in., and it would give more light than 5 ft. at the standard temperature and pressure. A second correction is therefore to be made in the illuminating power, and the value of the gas, the aforegoing example, would be reduced to 12·91 candles—

5·125 : 5 : : 13·24 : 12·91.

For the easy correction of volume, tables are to be used, which tables are identical with those published by Mr. Alexander Wright, for use with his specific gravity apparatus.

While admitting the correctness in theory of the referees' decision, it may, perhaps, be disputed by some persons whether this last refinement in photometrical operations is necessary or advantageous in determining the commercial value of gas. No doubt such corrections will sometimes put the public and sometimes the gas companies at a disadvantage.

It may, moreover, be said, that the commercial value of gas depends upon the light which it yields under actually existing circumstances, and not upon what it would give under theoretical conditions. The author, however, strongly inclines to agree with the referees' decision.

In the Metropolis Gas Act of 1860, it was enacted, that "Gas shall, with respect to its purity, be so far free from ammonia and sulphuretted hydrogen, that it shall not either discolour turmeric paper, or paper imbued with acetate or carbonate of lead, when those tests are exposed to a current of gas issuing for one minute under a pressure of $\frac{5}{10}$ of an inch of water. In respect to these tests it will be sufficient to say, that their efficiency depends upon the delicacy of the test papers. Those used by gas analysts and gas companies are generally of the best kind, but many so-called test papers are sold, which are almost worthless. It is quite certain that a good turmeric paper can be rarely exposed for a few seconds only to a current of coal gas without becoming somewhat discoloured, a circumstance to excite little surprise, when it is remembered how difficult it is to free gas entirely from ammonia, and that so small a quantity as one grain in 100 cubic feet of gas is said to be sufficient to affect a turmeric test.

The gas referees say that the gas shall be considered impure—rendering the company which issues it liable to the penalties described in the Act of 1868—when it contains more than five grains of ammonia in each 100 feet, or when it shows the least trace of sulphuretted hydrogen, and they have directed that all the [illegible] consumed in testing for illuminating power shall be [illegible] rough an apparatus in which slips of bibulous paper, [illegible] with a solution of acetate of lead, are suspended in a [illegible]e. This, as the author understands the instructions,

would give an exposure of more than three hours, and would constitute an efficient test for sulphuretted hydrogen.

With respect to ammonia, the test is to be a continuous one for a period of nearly twenty-four hours. The apparatus consists of two glass cylinders, in which are placed pieces of broken glass, or glass beads. The cylinders are connected together, and the gas operated upon is made to pass through both at the rate of not less than ·75 cubic feet per hour, the gas being measured by passing through a wet meter after it escapes from the cylinders. This course is adopted to prevent the possible absorption of ammonia by the water in the meter. Two test solutions are to be used, one of dilute sulphuric acid of such strength that ten measures will neutralise one grain of ammonia; the other, of a solution of ammonia, 100 measures of which contain one grain of ammonia.

The mode of testing is as follows:—A quantity of the dilute acid solution, in excess of any equivalent quantity of ammonia likely to be in the gas, as taken, and one half is poured into each of the cylinders. The gas is then passed through both cylinders. The ammonia will be arrested by the acid, and a quantity of the acid will be neutralised. From these cylinders the gas passes on to the sulphur test hereinafter described. At the end of about the twenty-third hour, the gas current is stopped, and the glass cylinders and the glass they contain are to be well washed with distilled water, and the washings are to be collected in a glass vessel. One-half is to be set aside for a week in a bottle, duly labelled. One-fifth of the remaining portion is to be taken in a separate vessel, and solution of hematine added till the liquor assumes a brown colour. An alkalimeter is to be filled with the test ammonia solution, and some of its contents is then to be dropped gradually into the portion of the washings operated on, the mixture being continually stirred. When the colour of the mixture changes to pink, the quantity of the test solution used is to be read off from the alkalimeter. That quantity will represent the amount of free acid left unacted upon in the cylinders, which, deducted from the original quantity put into the cylinders, will represent the amount of sulphuric acid acted upon by the gas.

If the gas is to be passed through the apparatus at the rate of ·75 cubic feet per hour for twenty-three hours, about 17·25 cubic feet will on every occasion be operated upon, and as the gas is to contain no more than five grains in 100 cubic feet, the maximum quantity in 17·25 feet should be ·8625 of a grain. Ten parts of the acid will neutralise one grain of ammonia, and therefore twenty parts of acid would give an ample excess. Suppose twenty

parts of acid be used, and the whole quantity of washings be afterwards operated upon with the ammonia solution, and it is found that 135 parts of the solution (100 parts of which contain one grain of ammonia) be required, it would show that ·65 of a grain of acid had been neutralised by the ammonia in the gas, and the quantity per 100 cubic feet would be 3·188 grains—

$$17{\cdot}25 : 100 :: {\cdot}65 : 3{\cdot}188.$$

As by the directions one-tenth part only of the washings is to be operated on, the quantity of test ammonia solution to give a similar result to the above would of course be only 13·5 parts.

The determination of the quantity of sulphur contained in purified gas, is surrounded with difficulty and uncertainty. Coal gas, however completely purified by the present known methods, contains bisulphide of carbon and some other compounds of sulphur, whose composition is uncertain or unknown. It has not been found practicable to determine the percentage of these sulphur compounds by any direct analytical process. The first part of the method of analysis generally adopted consists in burning gas and collecting the resulting products. There is no question that during the combustion of coal gas sulphuric and sulphurous acids are evolved, and that the latter acid is in the largest proportion. The great difficulty in the way of quantitative analysis lies in oxidising the sulphurous acid into sulphuric acid, and the uncertainty as to the accuracy of the results obtained is caused by the absence of everything like absolute knowledge as to whether the oxidising of the sulphurous acid has been complete, or to what extent it has been partially effected.

The first instrument which was devised for discovering the quantity of sulphur which gas contained in the form of bisulphide of carbon, &c., was produced by the late Mr. Alexander Wright in the year 1851. It is illustrated at Fig. 2, and consisted of a Liebig's condenser, in which a small jet of gas was burned, the air with which the gas was supplied being charged continually with vapour of ammonia. The products

Fig. 2.

[illegible] ustion, consisting of water, carbonic acid, sulphurous [illegible] phuric acids, together with ammonia vapour, nitrogen, [illegible] excess of air passed onwards together through the instru- [illegible] d it was supposed that the sulphur acids were com- [illegible] ested, and thrown down with the condensed water, in

combination with ammonia. This instrument was first used with a stream of icy cold water passing through the condenser—the maximum condensation being thus effected—but it was found after the lapse of some time, and with an extended experience, that the largest quantity of sulphur was rarely or never indicated when the largest quantity of condensed water was obtained, but that the reverse of this was the case. The question of the quantity of sulphur in gas was not then, however, considered so important as at a later period, and no very earnest attempts were made to fix the conditions under which the greatest quantity of sulphur could be obtained, or made apparent.

It may be supposed that when the Metropolis Gas Act of 1860 was passed, and it was enacted that the gas supplied in the metropolis should not contain more than 20 grains of sulphur in 100 cubic feet, no person doubted that Wright's and similar instruments afforded the means of arriving at the total quantity of sulphur in coal gas. But this opinion, if it existed, was certainly erroneous, and the gas companies of London and of other places have for years been struggling to attain to such a degree of purity in the gas as would meet the conditions imposed. Fortunately, their efforts have been tolerably successful, although, doubtless, the cost for purification has been largely increased. The gas referees in this matter, as in others, have power to establish the degree of purity of the gas; they have decided that Dr. Letheby's apparatus shall be used. At present they simply direct the tests shall be made and recorded, with a view, no doubt, to establish complete data, before deciding upon a standard of purity.

Dr. Letheby, on his appointment in 1852 as gas referee in the City of London, turned his attention to the question of sulphur in purified gas, and in the year 1853 or 1854 produced his apparatus, which since that time has been generally used. It is seen at Fig. 3, and consists, as now made, of a glass condenser, cylindrical in shape, with hemispherical ends provided with open necks, 2 in. diameter, and 2 in. long. The dimensions of the condenser are 4½ in. diameter, and 13 in. length.

FIG. 3.

The condenser is placed upon a wooden stand, which raises the condenser about 20 in. A trumpet-shaped glass tube, 3 in. wide at the base and $\frac{3}{4}$ in. wide at the top, where it is bent at a right angle, is fitted by means of a cork into one of the necks of the cylinder, and hangs with the wide end downwards. A tube $\frac{5}{8}$ in. diameter, and about 4 ft. long, bent at an angle, is fitted with a cork also into the other neck of the cylinder, and projects upwards. A Leslie burner is placed below the trumpet tube, and below that a bottle containing a quantity of the strongest liquor ammonia. This bottle is covered by an inverted glass funnel fitted with a tin tube, which passes upwards through the centre of the burner to about an inch above the jet holes.

When these instruments were first made, the same care was not taken, as at present, to preserve in each an absolute equality in the dimensions of the parts, or in the rate at which the gas was burned, or in the temperature of the rooms in which the instruments were used. It was found, however, that all these matters, or conditions, exercised considerable influence; the same gas burned at the same rate in instruments of different dimensions yielded in the analysis unequal quantities of sulphur, and the same effect arises when gas is burned in the same instrument at different rates of consumption. These variations are due, there is little doubt, to the different temperatures produced in the cylinder, and the degree to which the sulphurous acid is oxidised and combined with the ammonia. It is not probable that with any instrument of this character the whole of the sulphur in gas will be determined, but that is of little importance in a commercial test, compared with accordance in the results. If gas be purified to the highest degree which chemistry and practical experience show to be possible, it matters little whether the test which is used exhibits the whole, or a part only, of the percentage of impurity, providing the test be reliable and uniform in its indications.

The gas referees direct that the testings shall be made in a room where no gas is burned other than that which is being tested for sulphur and ammonia, and that in order to secure accuracy, this testing shall be made by two sets of apparatus simultaneously, and the average of the results so obtained shall be held to show the amount of sulphur contained in the gas; that the gas shall be burned at the rate of ·75 cubic feet per hour, and during a period of nearly twenty-four hours, at the end of which time the combustion of gas in both sets of apparatus shall be stopped simultaneously, the condensers emptied, and the condenser and tubes well washed with distilled water. As soon as this is ıe, the apparatus is at once to be set to work again. The ther operations for such apparatus is described as follows, l are the same as have been practised for some years past.

The liquor formed in the condenser, and the water used in washing the apparatus, shall be put into the same vessel. One half of the liquid so obtained is to be set aside and preserved for a week, in case it should be necessary to verify the correctness of the analysis. The other half of the liquid is to be boiled for ten minutes, and then heated with an excess of muriatic acid. An excess of a solution of chloride of barium is next to be added, and the solution again boiled for half an hour. The liquor is then allowed to cool, when the sulphate of baryta formed will settle at the bottom of the vessel; the liquor is then to be poured on to a filter paper, and the sulphate of baryta left thereon is to be well washed with hot distilled water, so as to perfectly wash away any of the muriate of ammonia or baryta. In order to ascertain that the ammonia or baryta has been wholly removed, a small quantity of the liquid should be placed in a test tube, adding a drop of solution of nitrate of silver; should the solution, instead of remaining perfectly clear, become cloudy, the washing must be repeated until no cloudiness is left. The filter paper and its contents are then made red hot in a platinum crucible, the paper burned to a white ash, and the resulting sulphate of baryta is to be carefully weighed; 117 parts of sulphate of baryta contain 16 parts of sulphur, and therefore, if 27 grains of sulphate result from the products of the combustion of 18 cubic feet of gas, the result would be equal to 20·5 grains of sulphur per 100 cubic feet for—

$$18 : 100 :: 27 : 150 \text{ and}$$
$$117 : 16 :: 150 : 20{\cdot}5.$$

The volume of the gas used in testing for ammonia and sulphur is also to be corrected for temperature and barometic pressure. The temperature and pressure are to be noted when the apparatus is set to work and when they are stopped, and the mean of these observations taken. This method of noting temperature and pressure is no doubt the best that can be adopted although not perfect.

The author has now completed his exposition of the methods which are practised on behalf of the public for determining the commercial value and the purity of coal gas, and he believes that there is no place in the world in which gas is subject to such rigid supervision and severe analysis as in the large towns in England. In France, the tests for purity go no further than the exposure of test papers to the gas. In some parts of America, gas is subjected to analysis possibly as severe as in this country, but in some countries no tests are made at all. If the gas consumers only realise the existence of rigid supervision, it is possible, that instead of so often finding fault with the companies for deficiency of light, sootiness of the gas, &c., they would turn to their fittings and burners, in the belief that they would there find the causes of dissatisfaction. The autho

thinks that if each gas consumer were to contract at a sum per year with a really competent gasfitter to look after the fittings and burners, in the same way as shopkeepers do with makers of measures with respect to scales and weights, &c., much or all of the inconvenience complained of would never be experienced.

The imperfections of part of the present system of gas testing have been mentioned, and this paper would scarcely be complete without some observations on the jet photometer, which has been proposed as a substitute for all photometers which depend on a comparison of flames: on Valentin's sulphur test, which it is believed renders the determination of the absolute quantity of sulphur in gas possible, and finally, on the modes of testing gas specially employed by gas makers, to satisfy themselves as to the quality of gas at various stages of its manufacture, and when sent out to the public.

The jet photometer is an instrument which is based upon a well-known fact established many years ago, but which received comparatively little attention until the late Mr. George Lowe brought it prominently forward, viz.: That the length of a flame produced by the combustion of gas from a jet under an exactly uniform pressure varies with the illuminating power of gas, the richer the gas the larger the flame. Jets provided, or in connexion, with graduated scales and pressure gauges, have been used in some gas-works for many years to show the quality of the gas during its production. These apparatus were generally wanting in delicacy in respect to measurement of pressure.

Fig. 4.

The present jet photometer (Fig. 4.) consists of a steatite jet fixed upon a very delicate King's pressure gauge, capable of indicating pressures to a minuteness of at least $\frac{1}{100}$ of an inch water pressure. The gas is made to pass through the gauge to the jet, and the pressure is regulated by a delicately acting dry or wet governor. The instrument is sometimes inclosed in a wood casing with a glazed door. A scale of inches and parts is engraved on the glass, and a corresponding scale in porcelain is fixed at the back of the cupboard inside. The base line of each scale coincides in level with the top of the jet. This plan of inclosure originated, the author believes, with Mr. Sugg. Sometimes the jet is surrounded by a glass chimney graduated in inches and parts. With these instruments twelve-candle gas issuing at $\frac{6}{10}$ pressure gives a flame 6 in. long. Mr. Bannister

suggested, in 1863, the possibility of making an instrument to register continuously, during a number of hours, the height of the flame; and Messrs. Kirkham and Sugg have since that time constructed a very beautiful apparatus in which the height of the jet flame and the acting pressure are registered by means of photography. Something of the kind had been attempted some years ago at the Great Central Gas Company's office, but was not successful. Messrs. Kirkham and Sugg's apparatus is exquisitely contrived, and reflects the greatest credit on its constructors. Such instruments have been proposed as constant tests of the illuminating power of gas, but the author thinks that they would scarcely satisfy the public. There is a definiteness and intelligibility about the present system which at once strikes the understanding of those to whom the system is explained, and the standard referred to, whether it be a candle or a lamp, is one which is familiar in every-day life. The length of a jet flame would have no significance to the public. A further objection is, that every instrument would, of necessity, have to be referred to one special officer, in order to secure uniformity in the sizes of the flames produced from various instruments, under certain specific conditions; and, lastly, there might arise irregularities in the results, or indications from possible changes by wear and increased friction in the gauge, governor, &c.

If a jet be used in public testing, the author believes an absolute measurement of the gas to be indispensable. Christison and Turner suggested, in 1825, the determining of the value of gas by the discharge of a specific volume from a gas holder, under a given pressure, through a jet—the height of the flame being constant, and the times for the consumption being accurately taken.

Mr. Valentin's method of analysis for sulphur is effected by means of the apparatus shown at Fig. 5, and is as follows:

FIG. 5.

A tube of platinum, about $\frac{5}{8}$ in. diameter in one part, and $\frac{3}{8}$ in. in another, and about 11 in. long, is partly filled, in the narrow part, with a closely fitting platinum wire cage (charged with spongy platinum), and partly, in the wide part, with soda lime (made by slaking pure lime with a solution, in distilled water, of pure caustic soda). This tube is placed in a gas furnace, and maintained at a high temperature, while a mixture of gas and air, in due proportions, is sucked, as it were, by means of an aspirator, through the tube. The due proportion of gas and air is shown by the absence of smoke-like fumes in the glass vessels which are interposed between the combustion tube and the aspirator, and also by an appearance of uniform heat in the tube. A deficient supply of air leads to local combustion, and the tube becomes hotter at the point where combustion begins. The mixture passes first through the spongy platinum, and is burned in the most perfect conceivable manner, thence the products pass through the soda lime, which absorbs every atom of the sulphuric acid generated, for it is believed that every atom of the sulphur in the gas is converted into sulphuric acid. Mr. Valentin has also used a porcelain tube, but then, instead of passing through soda lime in the tube, the products have been drawn through a strong solution of caustic soda contained in a glass flask. The soda lime in the one case is dissolved in dilute hydrochloric acid, and in the other case sufficient of the acid added to the soda solution. The sulphuric acid is afterwards precipitated with chloride of barium, and the further operations proceeded with as when Wright's or Letheby's apparatus is used. Valentin's apparatus gives a little more trouble than the others, owing to the necessity of using an aspirator, and it demands some delicacy of manipulation in its use.

Gas manufacturers employ for testing their gas:

1. The jet photometer, to indicate the quality of the gas as it is produced.
2. The Bunsen photometer, to test the value of the gas issuing from the works.
3. A specific gravity apparatus—not now much used.
4. Test papers for ammonia and sulphuretted hydrogen.
5. A eudiometer, to analyse the gas in its crude state for sulphuretted hydrogen and carbonic acid, and in its purified state for carbonic acid, carbonic oxide, and for hydrocarbon compounds, upon the volume of which last depend the light-giving value of the gas.

The first, second, and fourth tests have been already considered.

The specific gravity test is now considered valuable only as a means of arriving at the weight of the gas produced. It is

especially useful when testing the productions from coals, the weight of the gas added to the weight of the tar, ammoniacal liquor, and of coke, give the weight of the useful products, and this weight being deducted from the weight of the coal operated upon, the difference shows the loss arising from impurities and waste in manufacture.

Specific gravity apparatus are of various kinds; that at present most used by gas makers is Mr. Wright's; it consists of a light balloon, capable of holding one cubic foot of gas. It is first weighed when folded up and empty. It is then filled with gas, and as many grain weights (each of which is equal to the weight of 1·728 cubic inches of air at 60° Fahr. and 30 in. bar) are placed in the pan or car of the balloon as it will support. The total weight of the balloon and what it carries are deducted from the weight of a cubic foot of air, and the weight of the gas thereby obtained. Tables are used with the apparatus by which the volume of gas is reduced to standard temperature and pressure, its correct gravity determined.

The next most convenient specific gravity apparatus is Schilling's, illustrated at Fig. 6, which is based upon Bunsen's method. It consists of a cylindrical glass vessel and an inner movable glass tube, the latter fitted at its base with a metal foot or stand, and at the top with a metal cap, which is provided with a cock to admit gas, and with a central cock having two ways, one for the admission of air to the glass tube, and one for the discharge of air and of gas in the experiment. By this apparatus the specific gravity is determined by the times required for the discharge of equal volumes of air and of gas, through a minute orifice in a platinum plate mounted on the nozzle of the central cock.

Fig. 6.

The square of the number of seconds required for the discharge of the gas divided by the square of the number of seconds required for the discharge of air, gives the specific gravity of the gas. No correction is needful if the temperature of the air and gas be equal, about the securing of which there is no difficulty. If the time of discharge for air be 240 seconds and for gas 180 seconds, the squares would be 57,600 and 32,400, dividing the latter by the former ·562 would be the quotient, and the specific gravity of the gas,

FIG. 7.

The eudiometer shown at Fig. 7 is a glass tube about $\frac{5}{8}$ inch diameter and 30 in. long, in shape very like a walking-stick with a crook handle, the end where with a stick the ferrule would be is closed, while the end of the crook is open. The tube is graduated into parts each of which equal ·2 per cent. of its contents over a certain length. An upright cylindrical tank to contain water is also employed. It is a little deeper than the tube is long. In this tank the eudiometer is immersed for one minute before reading from the scale, so as to reduce the gas to a uniform temperature on every occasion.

The method of operating is the same whatever be the subject of investigation, and is as follows: The eudiometer tube is first filled with clean water, then a small flexible or a glass tube is passed into its open end and gas is allowed to enter until the eudiometer is charged to a slight excess, that is, the gas is to extend a little beyond the zero of the scale when the eudiometer is held upright. The small tube being removed, the eudiometer is immersed in the tank, which must have been charged with clean water, for one minute. It is then raised vertically until the water level within coincides with water level in the tank. If the water in the eudiometer be above zero, more gas is admitted; if the water be below zero, a portion of the contained gas is discharged by inclining the tube so that a bubble or two escapes. It is then immersed for a minute, and if when again raised the two water levels coincide with zero, the necessary re-agent is dropped into the open end of the tube, the opening is then tightly closed with the thumb or a stopper, and the tube well shaken.

Before removing the thumb the tube is turned, so that the end of the crook shall he the lowest point of the eudiometer, the water it contains falls over to the thumb, and the gas passes towards the closed end. The eudiometer is then brought into a vertical position, and in some cases partly immersed in water before the thumb is removed. The reduction in volume, if any, after an immersion of one minute, and when, as before, the water levels coincide, represents the volume of the substance sought.

There are certain special conditions which must be observed in using the eudiometer tube, and there are some little differences in the mode of manipulating, but it is needless to enter into their consideration in this paper.

The re-agents employed are:

Solution of acetate of lead for sulphuretted hydrogen.
" " potash " " carbonic acid.
" " bromine " " hydrocarbons.
" " bichloride of copper carbonic oxide.

By the aid of the eudiometer the gas maker ascertains the peculiarities of gases from different coals; finds the percentage of impurities to be removed, and thereby obtains data for estimating the cost of purification. It enables him also to determine to a considerable extent the efficiency of the purifying processes.

April 19*th*, 1869.

F. W. BRYANT, PRESIDENT, IN THE CHAIR.

THE METHODS EMPLOYED IN THE DETERMINATION OF THE COMMERCIAL VALUE AND THE PURITY OF COAL GAS.

DISCUSSION.

Mr. GORE opened the discussion with some remarks upon the subject of photometry. He observed that before the amount of light that gas would give could be precisely estimated, a standard must be fixed which approached as nearly as possible, in its chemical character, to the gas itself. The apparatus had been very beautifully described as consisting of the gas at one end burned in a certain form, and a candle at the other end, of a certain specific gravity, form, and character. In the interior of the measuring apparatus there was a box which contained a disc; and the equal illumination of that disc, at the point shown upon the scale of the bar, indicated precisely the relative quantity of the light obtained from the gas as compared with the candle. A difficulty presented itself at the outset, because all optical apparatus depended, to a large extent, for their accuracy, upon the visual organs of the operator. No two persons saw an object equally illuminated under the same circumstances. The eye of one person would be more easily affected by the quantity of the light, and the eye of another person by the intensity of light. If that was the case, the gas at the one side and the candle at the other were dealt with under those two conditions. The candle was essentially a light of quantity; the gas a light of intensity. If the rays of light from a body were those of quantity, they would have a yellow tinge. Rays of light issuing from the particles of carbon in the gas in a highly incandescent state, approached a blue tinge, or more closely to the chemical rays; and hence they were of a different character om the rays obtained from the candle. The consequence was at there was a source of error at starting, because the apparatus

was to determine the character of two lights from totally different points. But, further, the tube contained a very serious error, as the photometer was now constructed very differently from what it originally was by Bunsen, or as improved by King. In the box, in most instances, were two plane mirrors, placed at such an angle as to reflect the image of the disc into the interior of the box, and the eyes, looking as they did upon the whole reach of the photometer, saw two different images; and it was when those two images coincided, as nearly as possible, one with the other, that the apparatus was in the state of zero, or the two lights were equal. When light was reflected from plane mirrors, it involved another source of error, because the image produced by the light of quantity would be reflected in a different ratio from a light of intensity, and the error might be amplified in the standard itself, for this reason, that the candle consisted of two parts—the retort on one side, and the fuel from which the gas was to be generated in the state of spermaceti. The heat was generated in the wick by the combustion of the hydrogen, and hence the production of the gas. There was the process of the generation, first of a liquid, then of a gaseous compound produced from that liquid, and the decomposition of the gaseous compound, so that the carbon should become solid; and that was carried on in the candle, and therefore there was, in that case, a very variable quantity, because experiments showed that the facility with which that could be carried on varied very seriously in intensity, duration, and result. That arose from the way in which the candle was manufactured. In the case of a candle, the wick of which was closely packed or tightly twisted, a totally different result was obtained from that with a candle in which the wick was loosely packed, and where the quantity of fluid supplied by combustion was in a different ratio from that of the other candle. Hence another source of error was introduced, as a larger amount of fuel was consumed in one case than in the other. Besides, the character of the candle itself could scarcely ever be uniform, as they were more or less tapered; and just in proportion as the candle varied from a true cylinder, the rate of consumption would vary. That furnished another error in connexion with the use of the candle. If those errors were possible, he would ask if that was such a standard as the public should be led to consider as infallible? Having stated some of the objections to the use of the candle, he (Mr. Gore) would now refer to the lamp standard, which he considered preferable to the candle, as it was subject to less variation. The optical objection, however, remained precisely the same, with the exception that the lamp gave a light more approaching in intensity to the light which was being measured than the candle did. In Mr.

Keats's lamp, the oil was undergoing combustion in the best possible form, and the result of burning the oil under those circumstances with such a supply of air, more closely approximated to the gas burnt in the argand burner at the opposite end, and consequently the two lights for the purpose of comparison were placed in a better position than when the candle was used. But that standard was still open to the objection of impurity. Mr. Keats had stated his belief that sperm oil was the best fuel that could be employed, with which he (Mr. Gore) agreed, provided it was pure; but there was scarcely an article which was open to so much objection on the score of impurity and adulteration as sperm oil. If the fuel be not of a constant quality the same objection arose. As to the quantity of fuel consumed, it was still open to objection. A French chemist who investigated the subject showed, in connexion with the Paris mode of establishing a photometric standard of gas, that after some experiments with the moderator lamp, he found that with the lamp at zero, which was represented by one hundred, at the end of the first hour the amount of consumption was 103; at the end of the second hour it was 116; at the end of the third hour it fell to 110; and at the end of the fourth hour it resulted in 117. There was a difference of from 3 to 17 per cent. from the original standard of zero at which the lamp started, showing a considerable variation even in the fuel used under the same circumstances. The question of photometry, if it was to be brought into a practical form, would have to be reconsidered by those gentlemen to whom was delegated to a very considerable extent the question as applied to the metropolis; and what was done in the metropolis, would be extended throughout the country. Taking the very experiments and the explanation of Mr. Hartley, as given during the reading of his paper, and from the want of uniformity in the experiments which are recorded from time to time in the public prints, he (Mr. Gore) was convinced that none of the instruments or arrangements were of that perfect character which were necessary for determining the quantity or the illuminating power of gas.

Mr. Keats said that Mr. Gore had pointed out the principal defects in, and objections to photometry, as it at present existed, and he (the speaker) concurred in those observations. With respect to the defect of candles, they were themselves imperfect, and it was hardly possible to obtain satisfactory results from them. That circumstance led him to turn his attention to the lamp. H [illegible] proved the colza lamp to be a most imperfect instru- [illegible] burned very irregularly, and had many defects. The [illegible] reatest was that the supply of oil came over the level

of the edge of the wick-holder. That gave rise to another defect, which was that the lamp was regulated by the height of the wick, and that height rarely depended upon the height of that portion of it which was above the middle of the holder. The height of the wick was virtually changed by the oil acting in the way he had mentioned. Bearing that in mind, after some trials and failures, he hit upon the expedient of keeping the oil a short distance below the wick; and in the first experiments a row of holes was made about half an inch below the middle of the wick-holder, and through those holes the oil came. He used the arrangement of the common moderator lamp, and the oil was drawn over the upper edge of the wick-holder, but instead of passing through the extreme upper edge, it was made to pass through a row of holes a little lower down. That had been altered since by a very ingenious lamp-maker—Mr. Armstrong. In his lamp there were four slots cut in the wick-holder, and the oil passed through them. The changeable character of the glass was done away with, the glass being fixed close, as in a common argand lamp, and it might be raised or lowered like that in the common moderator. That did away with one element of uncertainty. His first experiments were made with colza oil, but that oil could not be looked upon as a standard of light which was unchangeable in its character. Colza oil, in the course of refining, was treated with strong sulphuric acid, and the dose was varied according to the character of the oil, and was regulated entirely by the judgment of the operator. The oil varied very much in character: there were many varieties of colza in use, and it was impossible to know what kind might be obtained. Then there was the age of the seed, and there were many other points which would affect the character of the oil. However, when the oil came into the hands of the experimenter it was treated with very strong sulphuric acid, and the quantity of sulphuric acid, and the mode of afterwards cleansing the oil, were left to the judgment of workmen. All those points were elements of uncertainty which entirely unfitted colza oil for use as a standard of light. He had therefore decided upon the use of sperm oil, as it was a natural product, and could be obtained pure, when it formed one of the most certain and unchangeable materials which could be used in a lamp. Of course sperm oil was costly, and was therefore subject to adulteration, but as regarded its use in photometry the cost was of no importance. He believed if it were necessary to employ pure sperm oil for this purpose there would be no difficulty in obtaining it pure. Whether his lamp was adopted or not, he believed that a lamp would ultimately be the standard for photometry. He did not think that Dr. Letheby's apparatus was

always very fairly treated. He had used it a great deal, and he found that his results were not in accordance with the published results of other observers. He found he got more sulphur than was stated in the published experiments. On several occasions where the published results were in accordance with the requirements of the Act of Parliament, he, upon examining the gas, found it ranging to about 25 grains of sulphur in the hundred cubic feet.

Mr. MORTON said he should be glad to learn the particulars of the standard which Mr. Fiddes, of Bristol, wished to introduce some time ago, using gas as a standard, burnt in an argand burner with an opaque chimney having a hole in it above the top of the burner. Mr. Fiddes had stated that the light projected through the hole was uniform whether the gas was ten or twenty candles.

Mr. OLRICK observed that he had taken notes of a few experiments which would show the great difficulty there was in obtaining proper results from a standard of 12-candle gas. The most remarkable results were as follows, the same gas being always used. With 129 grains of candle consumed the illuminating power was 12·6; with 145·2 grains consumed, it was 12·6; with 157·2 the amount of illuminating power fell even under that, and was 12·54. There was a good proof that with the same amount of illuminating power the consumption of grains of candle ranged from 129 to 157, showing that the amount of consumption was not in fair proportion to the amount of illuminating power in those three instances. There was another very striking example, that was, 130·2 grains were consumed when the illuminating power was 13·02. With exactly the same number of grains consumed the illuminating power was 15·44. It showed that with exactly the same weight of candle consumed the illuminating power varied over 2·2, but there were other instances where 134 grains were consumed, and the maximum was 18·18; and the variation between that and the other experiment was 2·7 candles, all reduced to the parliamentary standard. There was no doubt that those candles were not carefully selected, but at the same time the standard had been put so low that it was almost impossible to get candles which came under the parliamentary standard. Although the average amount was nearer 135 than 120 grains, it showed still that with the same amount consumed, the illuminating power ranged very widely. In all these examples the temperature was kept constantly at 62°, but no notice was taken of the barometer, and carrying on a number of experiments of that description was a mistake, if no notice was taken of the barometer. He (Mr. Olrick) did not consider Mr. Hartley's objection to the jet photometer a fair one, and thought it ought to be used.

Mr. HARTLEY said, in replying to the discussion, Mr. Olrick had said that he thought that the jet photometer a good instrument by which to determine the illuminating value of gas. He (Mr. Hartley) had stated some objection to the employment of the jet photometer as a standard measure of gas light in the paper which he had read. He believed it to be by far too delicate an instrument, and too liable to derangement, to be accepted as a standard instrument. With regard to purification, the gas companies in London were bound to use a purifying material, which was not the best which could be used. They were obliged to use it, because they were prohibited under penalties from creating any nuisance. If they were only to use lime, which was the best purifying agent, instead of oxide of iron, they might get rid of much more of the impurities from gas than it was possible to do with oxide. Indeed, the companies were even now driven to turn back in part to the use of lime. The difference in the quantity of sulphur in gas at different times arose no doubt from changes taking place in the condition of the purifying material now of necessity employed. That question, however, he had no wish to go into, because it had been dealt with in former papers, and was tolerably well understood. Mr. Olrick had referred to the experiments of Mr. Kirkham, and to the variations which that gentleman had found in the illuminating value of sperm candles. Mr. Kirkham no doubt went honestly to work, but it was to find defects in the candles, and it appeared that any sperm candle of the size called sixes was operated with, apart from any consideration of the manner in which it burned, and its consequent fitness for photometrical purposes. It certainly appeared to him (Mr. Hartley) that the extreme differences were obtained by a neglect or partial neglect of those precautions which a good operator would always regard. He believed further, that if they were to take examples from the various testing-rooms in London, and compare the results from day to day with the results of the tests at the gas-works, they would find little or nothing to indicate such extreme variations in the illuminating value of sperm candles as Mr. Kirkham had discovered. With regard to Mr. Fiddes' standard light, or rather lights, for there were two, he (Mr. Hartley) had tried them, but not with satisfactory results. One was a Bunsen burner surmounted by a piece of platinum wire. If he remembered rightly the burner was to be used in the following way: First the air openings at the base of the burner were closed, and the gas supply adjusted so as to give a luminous flame of a certain length, then sufficient air was admitted to destroy all luminosity in the flame. The platinum wire then became highly heated and gave off light, which Mr. Fiddes assumed to be constant

in quantity. As, however, the composition of coal gas was variable, it was evident that the heat produced, and consequently the light evolved, could not be constant. Moreover, the colour of the light, thus produced, was so intensely red as to render photometric comparisons almost impossible. Mr. Fiddes' second standard consisted of a jet or argand flame surrounded by an opaque chimney having a hole in it at a fixed distance from its base. Mr. Fiddes said that he had found that a uniform quantity of light was delivered through the hole although the quality of the gas varied considerably. As the length of a flame increased with an increase in the richness of the gas, a necessary consequence was, that the more highly illuminating parts of the flame were raised to a greater height from the base. Therefore it was not reasonable to expect that an equal quantity of light would be delivered through a hole at a fixed height in an opaque chimney, when equal quantities of gas of unequal quality were consumed. Nor did it seem more feasible that an adjustment of the flame to a uniform height at all times should give absolute equality in light. With respect to Dr. Letheby's sulphur test, he (Mr. Hartley) had no wish to find fault with the instrument, as it was the best in use. Mr. Valentin's apparatus was, however, much superior to Dr. Letheby's, but was delicate and somewhat troublesome to use. He believed that it might be modified so as to be easier to use, and still be capable of showing the total quantity of sulphur in gas. Dr. Letheby's apparatus, it was said, ought always to be used under the same circumstances, but there lay the difficulty—the controlling of circumstances. With regard to Mr. Gore's observations respecting differences between quantity and intensity of light, he (Mr. Hartley) would observe, that if almost the two extremes were taken—the electric light, and the light of an ordinary gas flame—there would no doubt be found such a difference between intensity and quantity, that it would be almost impossible to make photometric comparisons, but the difference in the character of a gas flame and that of a candle was exceedingly slight. In fact, for photometric purposes, *quantity* burners were used as distinguished from other burners, which were called *intensity* burners, the difference being simply that the quantity burner delivered its gas at a low pressure, and the intensity burner at a high pressure. If gas was burned under a high pressure a flame was obtained tending towards blue in colour, but the burners which were used gave a rather yellow flame, approximating to the colour of the flame of a candle. Some years ago he had a considerable objection to the use of reflectors, and he was induced on one occasion to make some experiments with a photometer having reflectors. He found there was a

great deal of comfort in using reflectors, and to satisfy himself as to whether there was a difference in the indications of the instrument with and without reflectors, he made a considerable number of comparative experiments, the results of which were in agreement, while the reflectors added to the comfort of the operator. Mr. Gore had spoken of the light of flames being due to the evolution of solid particles. That was the theory of Sir Humphrey Davy, he (Mr. Hartley) had believed, and until quite recently it was generally believed, that the whole of the light produced in flames was due to the evolution of carbon in a solid state, which carbon became highly heated in its passage through the flame; but he might mention that Dr. Frankland last year made some experiments on the combustion of gases, and had found that a gas (such as hydrogen) which had no illuminating power, under ordinary circumstances, was capable, when burned in a very compressed state, of being made to yield a considerable amount of luminosity, and Dr. Frankland's conclusions, as far as he (Mr. Hartley) could remember them, were that the light of a gas flame was really due to the combustion of true gases. Dr. Frankland did not attempt to deny that it was possible under certain conditions that solid particles were evolved and highly heated, but he thought it probable that with coal gas the greater portion of the light was due to the combustion of dense gases and hydro-carbon vapours. The imperfections of candles had been mentioned in the paper. As to the influence of the diameter of the candle, that was exceedingly variable. He had found sometimes that the upper portion of a candle would burn more than the lower, and sometimes the lower would burn more than the upper. There seemed no positive relation between diameter and rate of consumption, and therefore it was not safe to conclude that the lower part of a candle uniformly burned at a higher rate than the upper.

The PRESIDENT complimented the meeting upon the interesting discussion which had taken place. It was not only interesting, he observed, in a scientific point of view, but also of great public importance. The question of standards for the measurement of the purity of gas concerned not only London, but also all the great cities and towns in England and in the world. The members were certainly very much indebted to Mr. Hartley for the excellent experiments he had made. They had already given him a vote of thanks for his paper, but he, the president, would now call upon them to thank him for the interesting experiments he had shown them.

May 3rd, 1869.

F. W. BRYANT, PRESIDENT, IN THE CHAIR.

APPARATUS EMPLOYED FOR ILLUMINATION WITH COAL GAS.

BY WILLIAM SUGG.

THE subject of the present paper is a practical question to which the author has devoted some years of study, and which, he believes, will prove interesting not only to those members of the Society of Engineers who are professionally connected with the manufacture and use of gas, but also those who are brought into relation with it only as consumers.

The author desires in the first place to direct attention to a quotation from an old work upon gas which was published in 1841, twenty-eight years ago, but in that twenty-eight years what vast improvement has taken place in all matters relating to artificial lighting by gas. The production of a million feet of gas in a week, was then something to be pondered over, and now the requirements of London are moderately estimated at nearly 10,000 million cubic feet per annum. The quotation alluded to occurs in the first edition of Samuel Clegg's Treatise upon the Manufacture and Distribution of Coal Gas, p. 190. He says:

"While the engineer labours at the works to procure the most perfect gas from his coal, and relies upon the truth of his calculations for the quantity required by the consumers' lamps, he must not forget that the effect his operations will produce depends much upon the manner in which the gas is burnt. It is a simple theory involving only one question, but the results would make it appear to be both perplexing and difficult. Coal gas has now been used for the purposes of artificial illumination thirty-eight years, and the burners sanctioned by the companies at the present day are of the same shape, size, and uneconomical construction as those used in 1805. Unless some steps be taken

by the gas companies themselves to improve the system of burning, it may in all probability continue as defective as it is now for generations. The generality of consumers have other things to think of than the construction of burners, and manufacturers, although they may induce some few to adopt burners made upon correct principles, have not influence enough to introduce them universally.

"The engineers of companies are those with whom this must rest, and if they studied their own reputation and the proper interests of their establishments, the present description of burner would soon be abandoned, and those calculated for the complete combustion of the gas, substituted in their stead."

Thus speaks Samuel Clegg, and he then proceeds to tell us that the simple theory upon which to construct good burners is so to regulate the amount of air to support combustion, that it may be present only in sufficient quantity to take part in the elective affinity constantly going on between the elementary gases, viz., hydrogen, oxygen, and the vapour of carbon. He says, further, that as it is impossible to regulate the supply of air to such a nicety, "it is preferable that the air be slightly in excess, even to the diminution of the quantity of light than being deficient to produce smoke." How strictly the latter part of his advice has been followed, especially as regards the diminution of light, will be apparent, perhaps before the close of the present paper.

The burners spoken of by him are divided into the following classes, viz.:—The argand, the bat-wing, fish-tail, single jet, cockspur, double and treble jet, star, fan, and Scotch burners. Perhaps it is due in no small measure to the industry of some of our north country makers that England generally possesses such a collection of good burners for other gas than that they are supplied with, and the perseverance of these manufacturers has so influenced public opinion that the majority of consumers have, till of late years, been prevailed upon to accept these burners as proper for the twelve or fourteen candle gas with which England generally is supplied. Of this list of burners, there now remain in general use in England but three, viz., the argand, the bat-wing, and the fish-tail.

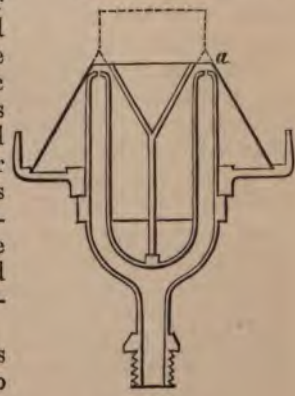


Fig. 1.

The general form of the argand is shown at Fig. 1; that was considered to be the correct burner in 1815, and was

arrived at, we are told, by Mr. Clegg, after a series of experiments conducted by Messrs. Clegg and Grafton. The gas to be consumed is made to arrive into an annular chamber, the top of which is pierced with holes by means of a bifurcated conduit, technically called "a crutch," from thence it issues to be consumed. Mr. Clegg describes it as follows :—" The burner itself was an argand, one inch in diameter within the drilled ring of jets with which a flame three inches high consumed 5 cubic feet of gas in an hour. For the complete combustion of that gas 50 cubic feet of air are required to regulate the admission of air to the flame, an exterior cone was supported upon the gallery bearing the glass chimney, the space at the top being $\frac{1}{26}$ of an inch from the edge of the burner, this annulus admits 30 ft. of air. The interior of the flame was supplied with air through the space left between the upper rims of an inverted cone and the interior edge of the lamp. This annular ring was $\frac{1}{20}$ of an inch wide, and supplied 20 ft. of air, so that the quantity was regulated to 50 cubic feet exactly. The combustion of the gas was further improved by the air being made to rush towards the point *a*, which it is evident would be its direction, and thus each atom of the elements of the carburetted hydrogen meets its equivalent of oxygen, and becomes entirely decomposed. The interior cone was made to adjust upon its supporting wire, and its annulus increased or diminished at pleasure."

Here, then, is the description of what was then, and doubtless would be now, a good argand burner, and if the principles thus laid down had been adhered to, and only the legitimate improvements upon their theory had been encouraged, the state of illumination by coal gas in the United Kingdom would be very different from what it now is. It appears that the pioneers of gas lighting considered the burner an important thing in the matter they had to carry out, and accordingly bestowed much time and talent in perfecting their models.

The burner just described was patented under the name of the double-cone burner, and the author now exhibits what he believes to be a slight modification of the original. It will be observed that the number and size of the holes is not given, but in giving the smaller sizes, it is stated that "The distance between the holes in the drilled ring should be so much that the jet of gas issuing from each should just unite with its neighbour, beyond this the number of holes is of no consequence, the size of the burner being computed from its diameter. The diameter of the holes should be about $\frac{1}{32}$ of an inch." Thus, the principles upon which to construct burners generally, and argands in particular, were considered definitely settled, and inventors and manufacturers set to work to improve upon their predecessors.

The author thinks, however, that there is some typographical error in the description of this burner, because the largest burners made for 5 cubic feet per hour, are not more than 1·1 in. across the outside of the chamber containing the holes.

Fig. 2.

Several modifications of this burner have since been produced at various times, but time will not permit of more than a mere glance at them, although some of them would afford useful instruction if they could be more leisurely examined. Among them are the chisel-shaped argand, by Clegg, shown at Fig. 2, and to which I shall again refer after my remarks on the argand burner. The Bronner burner is a much nearer approach to perfection as far as regards photometrical results. It is made very much like the Clegg and Grafton argand, but has its air passages protected by fine perforated metal.

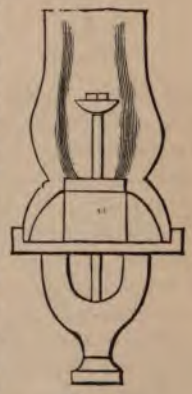


Fig. 3.

The national burner, and the Guise burner (the latter being shown at Fig. 3) are made upon the same principle, one being simply a modification of the other. The amount of light given by these burners as compared with the 14-candle Letheby, is shown on the Table, experiments C C and D D. We have here a modification of the Clegg and Grafton principle which deserves notice. By restricting more than they do, and lowering the passage which conveys air to the outside of the flame, and by placing a button over the central air passage, the inside current is impelled with a certain degree of force, and in greater quantity than would be required by the original arrangement against the cylinder of flame, which is thereby compelled to take a form somewhat resembling a tulip. This was in all probability an attempt to render the burner as shadowless as possible. The result of this improvement is destructive of illuminating power for 14-candle gas.

The Leslie burner (Fig. 4) introduces two new ideas, viz. :— The substitution of tubes for the chamber containing the holes, and the shortening of the chimney. The motive for the introduction of the tubes was probably for the purpose of obviating the shadow caused by the chamber. As in the national burner, the outer cone is suppressed, and the chimney contracted to

answer the purpose instead, and the otherwise too destructive effect of the large central air current is counteracted by a reduction in its velocity. Clegg and Grafton found by experiment, that the pressure upon the column of air set in motion inside an 8 in. chimney by the heat of the flame of their burner was equal to $\frac{3}{20}$ of an inch; the pressure upon that inside the Leslie burner must be much less. There is also a contraction in the top of the chimney which doubtless assists this result. There is yet another noticeable feature in this burner, and that is, that to insure uniformity in the make of the glasses they are blown in a mould. The late Mr. George Lowe informed the author that the shortening of the chimney of the Leslie burner was due to a suggestion made by himself to Mr. Leslie, consequent upon his observing the low illuminating power of the burner for the quantity of gas it consumed. The light was destroyed by too much air, and the reduction in the height of the chimney placed the Leslie burners, light for light, on a level with the original Clegg and Grafton burner. The author does not think it could have done more. By experiments made in 1859 by the late Mr. Henry Bannister, superintendent of the Chartered Gas Company, Westminster Station, it was found that the Leslie burner, with a 4 in. chimney, burning at the rate of 5 ft. per hour, gave the same amount of light as the Sugg 12-candle argand.

Fig. 4.

While these improvements were from time to time introduced, there had been a retrogade motion in the excellence of the manufacture of the Clegg burner. Contrary to the idea of Clegg and Grafton, burner-makers, gas companies, and gasfitters arrived at the brilliant conclusion that the number of holes had everything to do with the quantity of gas to be burnt. This settled the fate of the Clegg and Grafton argand, for very soon there appeared a variety of argands made in the original form, but with different numbers of holes, 5, 9, 10, 12, 15, 18, 20, and even 30 holes were used, and it was supposed that this number of holes represented, in some shape or form, the quantity of gas to be consumed by them, the lower numbers consuming the least gas. It is probable that the supply of gas on the contract system, to a considerable extent led to the perpetration of this error. A moment's consideration will show that in fixing the number of

holes in a burner, the size of the holes, and the diameter of the circle cutting these holes, is a very important consideration, that is, if the burner is to give light at the same time that it consumes the gas supplied to it. The author doubts not that at first both these points received some, if not thorough attention, but finally, the distinctions of sizes were lost, and almost every one of these 10, 15, 18, 20 and 30-hole burners might be obtained of several different sizes. In fact it was impossible to get any two burners alike, and it was the discovery of these differences which caused the author to turn his attention to the subject in 1858-9, and after some study and many experiments, he came to the conclusion that Clegg and Grafton were right in the main, and the author therefore made the first standard burner for common gas with 15 holes, substituting, however, a steatite top for the ordinary iron or brass top. This burner was first used at the Chartered Gas Works, Westminster, by the late Mr. Henry Bannister, and since that time it has been accepted as the standard burner for gas testing over a considerable portion of the globe, and has also been very considerably used by consumers. Fig. 5 shows the Letheby burner, which is the same —except in the size of the central aperture— as the original Sugg 12-candle burner.

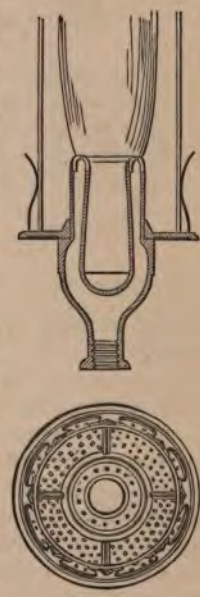


Fig. 5.

It has been a very generally received opinion, and it is stated by Clegg, that the body of the burner should be long between the stem and jets, so that the gas may become heated as it passes to be burned. The author has had reason to alter his views upon this point, and he is now of opinion that the contrary is the best, and that the gas should arrive at the point of ignition at the temperature of even 58 deg. Fahrenheit, if possible. He has also found that the highest illuminating power is to be obtained from gas burnt at the lowest possible pressure, as low even as the $\frac{1}{100}$ of an inch of water, whilst on the contrary, the smallest amount of light co-exists with the highest pressure at the point of ignition. Further, it is important that the flow of gas to the point of ignition should be easy and uniform, and it is of consequence also that the surfaces with which it comes in contact immediately before ignition should be smooth.

A combination of these principles with those already laid down by Clegg and Grafton, has resulted in the production of

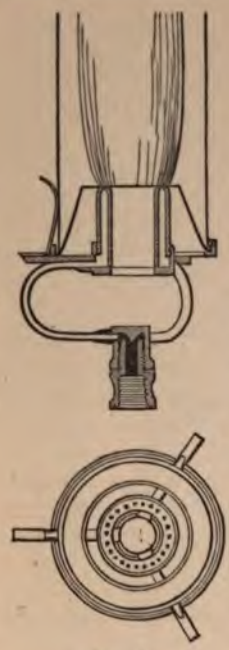


Fig. 6.

the London burner, shown at Fig. 6. It will be observed that the communication between the chamber containing the holes and the stem, is made by means of three small tubes, and in order to insure the issue of the gas at an imperceptible pressure, the sum of the area of the holes for the egress of the gas at the point of ignition is much greater than that of the area of the three tubes which conduct it to the chamber. This chamber is made of steatite, which is a non-conductor of heat, so that the lower part of the chamber, which is made of an alloy of tin, is always kept cool. The contrary would be the case if this chamber were made of brass or iron, for in about ten minutes or a quarter of an hour the soft metal would be melted, and this the author considers a sufficient proof of the unsuitability of iron or brass for gas burners. It is the opinion of the author that, increasing the volume of gas by heat is equivalent to diluting it, because the amount of carbon vapour is spread over a larger area, and that it is most pernicious to gases of low, but less so to those of high illuminating power.

It appears to be the prevailing idea that it is a matter of no consequence what kind of burner is employed for the combustion of gas, and consequently the desideratum is cheapness. This has materially influenced the use of the argand burner, for the impression being that there was more heat and smoke, and less light produced from the argand—which, according to the popular notion, must consume a much greater quantity of gas than either the fish-tail, or the bat-wing—it is not to be wondered at that the latter have become the rule, and the former the exception, more especially when the cost of fish-tails or bat-wings is less than the cost of the chimney of the argand. It must also be borne in mind that argands have been, and still are made so badly, that when tried under the best conditions in a photometer with regulated pressure, many of them do not give much more light than common iron fish-tails, and not so much as some of the numerous regulated and patented economising burners. Table I. shows the difference between the amount of light to be ;ained from five cubic feet of gas when burned by different ;and burners in a photometer room, the gas being constant

at 14·0 candles as tested by the 14-candle Letheby standard burner. The difference between the highest and the lowest is 13 candles. These differences, although great, are not greater than in actual practice, for it must be borne in mind that this is the maximum amount of light which can be got out of these burners, and they are all what are considered to be well made burners, and new, and some of them are not incorrodible. They were provided with suitable chimneys. Another popular idea with respect to the argand burner is, that the higher the chimney the better light the burner will give, and we have only to look at the numerous outside lights in London to see what lengths chimneys can be carried; 11 in. is a common length for those, and 8, 9, and 10 in. for shops and offices. The effect upon a 24-hole burner, as shown in Table I.; experiment G G, is about one candle less for every inch of increased length of chimney. From this it will not be difficult to form an idea how much we have progressed in gas lighting since Clegg wrote the words quoted at the commencement of this paper as far as relates to argand burners for common gas. The cannel gas argands have always been, and are now, in a worse position than those for common gas, for by some means which the author cannot explain, gas engineers, and burner-makers too, are wedded to the idea that for this gas the holes in the burner cannot be too fine.

The burners in experiments L L, R R, B B, and D D, Table I., will burn a greater quantity of gas than 5 ft.; but it will be seen that the percentage of light per cubic foot consumed, is no better than with 5 ft. per hour, they give the same amount of light as the standard at a cost varying from 6·3 ft. to 10 ft. per hour.

The single, double, and treble jets, the fan, and star burners, need no comment, as they were soon found to be unsuitable for light giving purposes, and consequently earned the condemnation of the trade and consumers generally. The fish-tail, Fig. 7, occupies the most important position of all the burners in England, for nearly one-half of the gas made is burned through them, and they have occupied the attention of the inventors of all kinds of gas improvers to a surprising extent. Referring to Fig. 2, the chisel-shaped argand by Clegg, the principle upon which the fish-tail is constructed may very plainly be seen. It consists in making two currents of gas strike together at an angle which causes them to flash out against each other, producing that well known flame which is supposed to bear a likeness to a fish's tail. The

Fig. 7.

angle at which the two currents of gas impinge upon each other is the most important point in the manufacture of the burner next to the size of the apertures from which these currents issue. If the angles are acute the illuminating power of the gas is greater, but the burner will stand less pressure without roaring or screaming. On the other hand more obtuse angles reduce the illuminating power, and enable the burner to stand more pressure without the objectionable sounds. The author is speaking of this burner as applied to common gas of twelve or fourteen candles. Richer gases admit of different treatment in the arrangement both of holes and angles.

To combine harmoniously these two opposing principles has been the aim of the numerous inventors of gas economisers, and apparently there is a large field for the still further exercise of their ingenuity, and a tolerably fruitful source of revenue to the Patent Office. The desideratum would seem to be to make a fish-tail with holes large enough, and inclining at a proper angle towards each other, so as to give the best light, stand the varying pressures of the street mains without roaring, and for this purpose various kinds of moderators, regulators, economisers, perfecters, and numerous other things, have been brought before the public. Fig. 8 shows the gas light monitor, in which the gas is made to blow upon the under side of a gilt disc of brass having a central aperture in it. When at low pressure and with the plate resting, the gas issues out of a number of small holes pierced round the rim on which it rests. Under high pressure the plate is blown up to the upper rim, and all egress for the gas is stopped except through the small hole in the centre of the plate. This was one of the first, if not the first, of these inventions. Fig. 9 shows a chamber filled

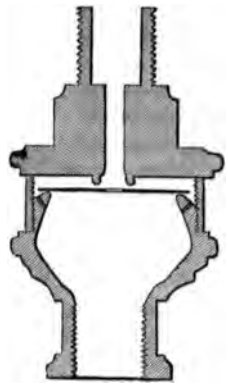


Fig. 8.

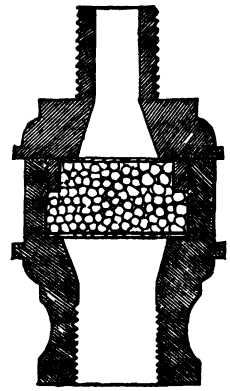


Fig. 9.

with either shot, cotton, horsehair, wool, iron borings, &c., and which contains the principle of many other patents.

The Bronner burner, Fig. 10, exhibits one of the latest modifications of another class of economisers, which gives some of the best results. The principle consists in allowing the gas to pass first through a small burner into a chamber fitted at the top with a much larger one, from whence it issues to be burned at a considerably lower pressure, and consequently with an improvement in the amount of light given. Table IV. shows the improvement which has been achieved over the ordinary ungoverned fish-tail, but it is necessary to direct attention to this fact, that the obstruction caused by the economiser will materially affect the question of pressure and leakage in mains.

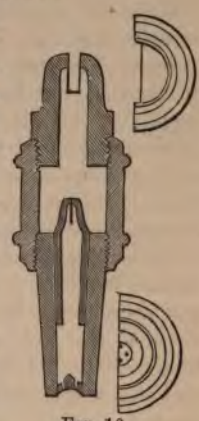


Fig. 10.

The cone shown at Fig. 11 gives the best result with a large fish-tail, and requires only a pressure of $\frac{6}{10}$ths to do it, but will not work satisfactorily without the pressure is constant at the meter.

Fig. 11.

Table No. III. shows the results given by a number of fish-tail burners procured from consumers, and from some of the best shops in various parts of London, and the author thinks that a perusal of the table will show that for common gas at least the fish-tail burner is a costly mistake. They are the cause of the constant dissatisfaction expressed by gas consumers, both as regards the quality of the gas supplied to them, and the quantity for which they have to pay. Were they banished an enormous improvement would soon manifest itself in the amount of light obtained from the gas. It must not be understood that the author would suggest the abolition of sun burners and other such arrangements, which are much admired for lighting public rooms, and for which the fish-tail is suitable, because in speaking of these the question of cost of gas must not enter into the calculation. A certain effect is required in light and ventilation, and that achieved the value of both must be taken together, but for the ordinary purposes of life, the question is widely different, and the problem to be solved is, to obtain the greatest amount of light per cubic foot of gas burnt with the greatest possible comfort. This the author submits is not to be done with the fish-tail burner as at present constructed. If a flat flame of the double-current class is desired, the author thinks

that Clegg's burner, Fig. 2, with a proper glass which might be contrived, would give much better results in every way.

One other error is constantly committed with the fish-tail burner, and this is, that when they are used with glasses or "moons," as they are technically termed, the holes in the bottom of these moons are narrower than the width of the flame, consequently the sharp current of air induced by the burning gas folds the sides of the flame together, which is likely to produce smoke, and certainly makes the flame unsteady, Another error is made by designers of gasaliers, who mostly place the cocks immediately under the burners, so that the quantity of gas issuing from the regulated aperture has not space or time to diffuse itself before it reaches the burner. Thus a current of gas very often of high pressure is projected either against the sides of the burner, or directly up to its orifices, a method of treatment which seriously interferes with the illuminating power, and the tranquillity of a fish-tail burner much more than any other.

As an example, a No. 8 steatite burner fixed close to the cock in the manner usual with a vast number of gasaliers, would, with an inch pressure in the gasalier, burn only five cubic feet per hour with an unsteady light, while on a cone as shown at Fig. 11, it would burn steadily up to seven cubic feet per hour.

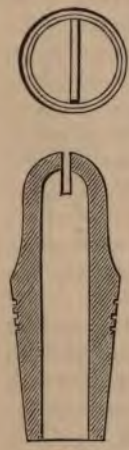


Fig. 12.

The bat-wing burner shown at Fig. 12 consists of a cylindrically shaped piece of material, closed at one end with a circular top, into which a slit is cut; from this the gas issues, producing a flame when lighted similar to the extended wings of a bat. The size of the slit and the shape both of the inside and outside of the top, are the important points to be observed in making bat-wing burners. These attended to, it is possible to produce a useful burner for all qualities of gas suitable for street lighting, or for public rooms, or, in fact, wherever it is desired to burn gas without glasses. Properly fitted and of the proper size, the author considers that they would most advantageously replace fish-tails upon gasaliers, &c. Their value as illuminators is shown conclusively in Table No. II. It will be observed that referring to the Table of Economisers, the best result is achieved by the bat-wing with a good flame, while the fish-tails lose at their best 2·86 per cent. of light with a very unsteady flame. It must be mentioned, however, that the best bat-wing flame is very susceptible to draught. Referring to experiment, C Z, in the bat-wing table (No. II.), it will be seen

that the steatite bat-wing loses only 7·14 per cent., the Amsterdam, A Z, loses 8·58, and G Z, No. 6, loses 14 per cent. These three burners are of the same pattern, being that made by the author for the public lighting of the city of Amsterdam. The No. 6 is that best suited for use where there is a draught, and it is made use of without a glass. The two experiments marked F Z, and E Z, are inserted to show the effect of the diminution of the width of the slit upon the amount of light given by a fixed quantity of gas, which diminution may be carried on till nearly the whole of the light is destroyed. The last experiment, H Z, should not be lost sight of, as it was made with one of the iron bat-wing burners, which were used for public street lighting before the introduction by the author of the steatite bat-wing, which is rapidly superseding it, and which when fixed on a governor in the manner now so generally adopted, gives the next greatest amount of light for the quantity of gas consumed to the Letheby argand burner. By consumers this kind of burner (the iron bat-wing) is still employed to a small extent, because they are under the impression that the finer slit consumes less gas, but the fish-tail is the favourite burner.

In conclusion the author would observe that the experiments in the tables showing the results obtained from the consumption of the several quantities of gas by the aid of fish-tail and bat-wing burners were made with the flat of the flame facing the photometer disc. Therefore in comparing these results with those obtained from argands, a reduction must be made from the results of the two former, because while the light emitted from the argand is in every direction the same, that from the fish-tails and bat-wings is not so great from the edge as from the flat of the flame. This allowance will vary according to the width of the flame and its thickness from 15 to 25 per cent. A careful comparison of all these circumstances will lead to the conclusion at which the author has long since arrived, viz., that, properly fitted, the argand burner will be found to be, all things considered, the best and cheapest for the general consumer, notwithstanding that its first cost is much greater than that of the bat-wing and fish-tail, and that for all other purposes to which the argand is not admissible, the bat-wing is the most suitable. But whether the one or the other of these burners is employed, the pressure at the meter should be maintained perfectly uniform, and the difference of pressure in various parts of buildings, however caused, whether by differences of level or of draught, should be compensated for by a fixed independent regulation of each light, and not as at present by the differences in the sizes of the aperture for the escape of the gas to be consumed.

No. I.

TABLE of the Amount of Light in Standard Sperm Candles given by different Argand Burners (such as are in use by Consumers in London and the Provinces) burning Gas which, being tested by the Letheby Standard Burner, was equal to 14 Candles.

Experiment.	Description of Burner.	Consumption per hour.	Pressure at point of ignition.	Illuminating power in candles.	Percentage of loss of light.	Remarks.
		ft.	in.			
L L	Shadowless argand .	5.0	0.32	8·80	37·16	7 in. chimney.
	Same burner . .	6·3	0·47	14·00	,,	7 ,,
A A	Shadowless argand, patent with gauges .	5·0	0·318	12·20	12·86	
R R	Albert iron top . .	5·0	0·23	8·10	42·19	8 ,,
	Same burner . .	6·3	0·33	14·00	,,	
S S	Short argand, 5 chimney	5·0	0·11	9·00	35·70	
C C	Small Albert, with button and glass cone . .	5·0	0·18	3·00	79·52	
F F	15-hole iron top argand .	5·0	0·28	5·90	58·14	
K K	Platow's double cone .	5·0	0·11	6·90	50·76	
B B	Large Albert, with button and glass cone . .	5·0	0·19	5.0	64·51	
	Same burner . .	7·5	0·35	14·0	,,	
D D	Large burner, with button and outside cone .	5·0	0·11	1·0	93·45	
	Same burner . .	10·0	0·80	14·30	,,	
G G	24-hole iron top . .	5·0	0·36	8·94	36·23	7 in. chimney.
	Same burner . .	5·0	0·36	7·90	41·9	8 ,,
	Same burner . .	5·0	0·36	7·20	43·9	10 ,,
X X	Universal argand . .	5·0	·4	7·0		
	Same burner . . .	6·3	·6	14·0		

No. II.

TABLE showing the Amount of Light in Standard Candles given by different Bat-wing Burners (such as are in use among Consumers in London and the Provinces) burning Gas which, being tested by the Letheby Standard Burner, was equal to 14 Candles.

Experiment.	Description of Burner.	Consumption per hour.	Pressure at point of ignition.	Illuminating power in candles.	Percentage of loss of light.
		ft.	in.		
C Z	Bat-wing steatite . . .	5·0	·25	13·0	7·14
A Z	" Amsterdam . .	5·0	·26	12·79	8·58
G Z	" No. 6, common . .	5·0	·38	12·04	14·0
F Z	" No. 3, " . .	5·0	·65	8·0	42·9
E Z	" No. 4, " . .	5·0	·72	9·0	35·70
H Z	" No. 5, iron ordinary street-lamp burner.	5·0	1·15	5·41	60·16

No. III.

TABLE showing the Amount of Light in Standard Candles given by different Fish-tail Burners (such as are in use by Consumers in London and the Provinces) burning Gas which, tested by the Letheby Standard Burner, was equal to 14 Candles.

Experiment.	Description of Burner.	Consumption per hour.	Pressure at point of ignition.	Illuminating power in candles.	Percentage of loss of light.
		ft.	in.		
O	Fish-tail, No. 3, iron . . .	5·0	1·12	5·50	60·97
G	" No. 3 . . .	5·0	1·20	4·53	68·5
M	" No. 3 . . .	5·0	1·125	6·92	50·76
Q	" No. 4 . . .	5·0	1·20	5·97	58·10
F	" No. 4 . . .	5·0	0·69	9·36	33·14
E	" No. 4 . . .	5·0	0·57	9·00	35·7
D	" No. 4 . . .	5·0	0·90	6·13	56·17
A	" No. 4 . . .	5·0	0·52	11·10	20·74
B	" No. 4 . . .	5·0	1·00	5·41	60·16
C	" No. 4 . . .	5·0	1·00	5·50	60·97
H	" No. 5, adamas . .	5·0	0·66	9·00	35·70
K	" No. 5, iron . .	5·0	0·80	7·00	43·66
J	" No. 5 . . .	5·0	0·80	6·90	50·76
L	" No. 6 . . .	5·0	0·55	10·50	25·00
N	" No mark . . .	5·0	0·605	10·00	31·43
P	" No. 2, iron . .	5·0	1·65	3·97	71·90
R	" No. 8, steatite . .	5·0	0·40	12·00	14·28

which seemed to be specially constructed to produce intensity and not quantity of light. Such burners were not good. Neither were those in which the development of quantity of light was carried to an extreme degree, for such burners were extremely liable to smoke. He (Mr. Hartley) thought it better to sacrifice a little light, than to use burners which were unduly liable to smoke. In his (Mr. Hartley's) opinion, a consumer of gas had better use a flat flame burner, requiring a pressure of ·4 in., than one requiring a lesser pressure, that was if the consumer wished to avoid smoke, because the variation in the quantity and in the direction of the air supplied to a low-pressure burner caused the flame to flicker and to smoke.

Mr. Harris said he agreed with Mr. Sugg in his remarks respecting burners. If the principles the author had stated were carried more generally into practice, complaints which were now continually made with respect to the supply of gas would cease. Consumers had not so much the power of selecting their burners, as the gasfitters who supplied them. The subject of the quality or description of burner recommended was one which had not been taken into proper consideration by gasfitters, but which nevertheless required their careful consideration. He had found by experiment that the largest amount of light was produced from gas when it was burning at the lowest possible pressure. There was a limit to which that pressure might be reduced, because when too low the flame commenced smoking; therefore the proper medium was that of a gaslight which was burning in such manner as to be just under the point of smoking. It was difficult to regulate the pressure at which gas was burning, but it might be done by means of efficient regulators.

Mr. Gore said he could not agree with Mr. Sugg that so low a pressure as that mentioned by him could place the gas in circumstances in which it gave the largest amount of light of which it was capable. There must be a certain amount of pressure to project the gas into the atmosphere. A very low pressure under all circumstances was hardly the most economical and beneficial mode of burning gas. He agreed with the author of the paper that an excess of pressure gave a result in an opposite direction detrimental to the quality and quantity of the light obtained from it.

Mr. Wright said that if all the carbon was taken up when the gas was burnt, there was less light than otherwise. It appeared desirable to see what was the residue of combustion, or what gases resulted when the greatest quantity of light had been obtained; if the gases were heated the quantity was generally augmented, although he doubted whether there was any advantage in so doing. Incandescent gases were very slightly

luminous, consequently if the carbon was volatilised a blue light was obtained. There was a large amount of heat, but very little light. He therefore thought it would be best to endeavour to ascertain what was the residue of combustion, and what the gases were composed of after they were burnt. That would lead to a determination of the best conditions under which gas should be burned.

Mr. William Adams said he had recently tried a few experiments with Brönner's burner, and the result showed an economy of about 23 per cent, as compared with the fish-tail burner, and with an equal amount of light. There was a curious circumstance connected with the use of those burners, and which illustrated the principle mentioned by Mr. Sugg. One of the burners was tried near Broad-street Station, where the gas was laid on direct from the Chartered Gas Company's holder, and the pressure there was unusually great. There was nearly five inches of pressure, but the Brönner burner did not roar nor make any noise, and the light was perfectly steady, thus illustrating the correctness of the principle.

Mr. Olrick observed that the gas companies ought to supply gas at an uniform pressure, and if they could not maintain an uniform pressure in their mains, they should supply each consumer with a governor. He suggested the more general use of a thin piece of platinum fixed over the fish-tail burner, which expanded the flame and greatly improved the illuminating power, to the extent—according to Dr. Letheby—of 55 per cent. If the cone referred to by Mr. Sugg was movable instead of being a fixture, he (Mr. Olrick) thought it would give additional uniformity.

Mr. Alfred Williams observed that it was to the interest of the gas companies to keep the pressure as low as they could. Mr. Olrick had referred to Mr. Scholl's burner, which had a piece of platinum attached, and which had a very extraordinary effect on the burner. He (Mr. Williams) believed that the contrivance failed, because the platinum broke, and as they cost one shilling to be renewed, it proved rather an expensive burner. He had seen Mr. Sugg's burner, and thought it was the best burner that could be had at the present day.

Mr. Sugg in reply upon the discussion said, Mr. Hartley had mentioned the Frankland burner, which was simply an argand burner with a chimney on the outside of the ordinary chimney, so that the air admitted to the burner had to pass over the top of the outer chimney and down between the two, and thus get heated on its passage to the gas. He had tried a great many experiments with the Frankland burner, but he had never succeeded in obtaining anything like a good result from it. The

London burner was constructed upon calculations and from experiments made with other burners. He (Mr. Sugg) had never tried any experiments for illuminating power with the burner itself, but the results arrived at by independent testers had shown it to be about 18 per cent. better than the Letheby burner. Referring to the first portion of the paper, it would be recollected that Mr. Clegg said that the way in which the burners were to be made was such as that the air must be admitted to the burner in proper quantity—just the amount to consume the gas. As far as that was concerned, he (Mr. Sugg) had tried some experiments with Mr. Clegg's double-cone burner, and he thought there must be some mistake in the sizes mentioned. because, making it of exactly those sizes, he had never been able to get so good a result as it was evident from the description would be arrived at. The admission of the air exactly in the quantity required was not a correct theory. If the air contained nothing but oxygen, it would be, but it contained other substances, especially nitrogen, and a column of air must be allowed to pass through the burner, so that the oxygen would combine by its own affinity with the hydrogen to be consumed, and the nitrogen pass away. In the London burner the gas arrived through the tubes at a pressure of about four or five-tenths of an inch, which was regulated by the inverted cone. The holes at the top of the burner were made of such a size that the gas would issue from the combustion chamber of the burner at a greater rate than that at which it would enter. Consequently the pressure was *nil*, and it was not necessary to a burner that there should be pressure upon it in order that it should burn well. A burner of that kind where the gas issued almost by its own gravity did not appear, by experiments made, to be more liable to smoke than a burner working at a higher pressure, but if an argand burner was worked at a higher pressure than that, light would be lost, and the higher the pressure at the point of ignition, the lower the illuminating power. The explanation was, that if the gas was driven out at a great velocity, it was shot, as it were, out at the top of the chimney before it had time to combine with the oxygen admitted to both sides of the flame, and to get into perfect combustion. Mr. Hartley said it was better to sacrifice a portion of the light than to have a smoky burner. It was not at all necessary to have either one or the other; light need not be sacrificed in order to prevent . If the gas was perfectly consumed and combined with ygen in a proper manner, perfect combustion ensued, and vas no smoke. That result was obtained perfectly with rand burner, but not with any other. It was supposed fish-tail burner did not produce any smoke, but with

the least draught the flame would fall together, and there would be smoke. With regard to the cone of the London burner, the higher it was screwed up the more it closed the opening, and the amount that it should be screwed up or down was regulated by paper washers inserted at the joint of the regulating cone. When it was screwed down so as to get exactly the flame required about midway up the chimney, there was no smoke, and in that state the burners could be turned full on, and the light was always uniform with a perfect combustion. Mr. Harris said that burners caused a great deal of dissatisfaction on the part of the consumers. He (Mr. Sugg) agreed with that statement. But consumers generally applied to the gas company, when they found their bills were higher than they considered that they ought to be, and the usual rule was for the company to recommend a smaller burner. The consequence was that there was no decrease whatever in the consumption. The result obtained from the platinum perfector depended upon the size of the orifice. If the gas was turned on at a high pressure, then the platinum perfector reduced the destructive effect, and prevented the gas from being driven out so forcibly, thus giving a better result. A fish-tail burner was always taken for experiments with the platinum perfector, but if a burner with a large orifice was used it would not give any better result with the perfector than without it.

The PRESIDENT said it was apparent from the opinions of those present that a fish-tail burner was about the worst kind of burner, and the sooner it was abolished the better. The London burner appeared to be the best he had seen. The gas companies supplied meters, and he thought it would be to their advantage and to the advantage and satisfaction of the public generally if they were to supply good burners. It would doubtless be more economical to the companies, and it would give consumers more satisfaction if the companies were to supply burners which would give the best light at the most economical rate.

October 18*th*, 1869.

F. W. BRYANT, PRESIDENT, IN THE CHAIR.

ENGLISH AND CONTINENTAL INTERCOMMUNICATION.

BY PERRY F. NURSEY.

ONE of the most prominent and important questions of the present day, and one to which a very wide-spread attention is being given, is that of providing a more easy and rapid system of intercommunication between England and France than we at present possess. Nothing is more obvious than the necessity which exists for improvement in this direction. The steam-packet service between England and France—which is in fact a service between Great Britain on the one hand and Europe and the East generally on the other—is about the worst we have. It is an undoubted fact that the great majority of passengers crossing the Channel suffer—short as is the sea passage—extreme discomfort from sea-sickness and want of shelter during bad weather. Not unfrequently the traveller from India or from America finds the British Channel the most unpleasant part of his journey, and he sometimes looks forward with more anxiety to the state of the Channel than to the heat of the Red Sea or the passage of the Atlantic. In the case of America, he will often select a French or an English vessel, according to the country which he desires to reach, in order to avoid the Channel crossing, rather than from any other consideration. There are now, exclusive of the routes by Ostend and Antwerp, 310,000 passengers per annum to be provided for, of whom 142,000 travel by Calais, as the mail route. Those numbers are undoubtedly capable of great augmentation on greater facilities and increased comfort being afforded. But the existing steam vessels, restricted as to their dimensions for want of better pier and harbour accommodation, are disproportionate to the service.

Larger vessels, with less movement in rough weather, more shelter, and better accommodation generally, would do much to mitigate the discomforts of the sea passage. Pending the successful issue of other more extensive and more permanent projects, it would be well if these improvements were effected. But larger vessels cannot be employed for a fixed service until better provision is made for embarking and disembarking passengers, especially on the French coast. The Society of Arts, ever on the alert to improve the social position of mankind, has offered its gold medal for the best design for a steamer which shall afford the most convenient shelter and accommodation to passengers on the deck of the vessel crossing between France and England. The attendant circumstances of the case, however, oblige the society to specify that the steamer is not to exceed in tonnage and draught the best vessels now in use between Folkestone and Boulogne. There have also been other schemes propounded at various times for vessels of special build suited to the present harbour accommodation. Into these propositions, however, it is not our present purpose to enter. The growing necessity for a radical improvement teaches us that they can only be regarded as temporary expedients, which must give way in course of time to more permanent works. We will therefore proceed to the consideration of the more important and extensive projects which have been placed before the public during the last few years. These consist of tunnels driven beneath the bed of the sea; submerged roadways and tubes; large ferry-boats carrying the trains on board, bridges across the Channel, and embankments.

If we glance at the history of the question we shall find it to extend further back into the past than is generally supposed. Something like a score of projects for effecting direct communication between the two countries have been proposed from first to last. Up to the close of the year 1866, three French projectors had proposed tunnelling under the Channel; five English and two French inventors had proposed submerged tubes; one Frenchman proposed an arched roadway or tunnel, and an Englishman a bridge. Since that time there have been some seven or eight proposals for effecting that object; to these attention will presently be specially directed. The idea of tunnelling is much the oldest; a French engineer, M. Mathieu, nearly seventy years since, considered the scheme practicable. He worked out the details of his plan and laid them before Napoleon, then First Consul. In 1856, M. Thomé de Gamond proposed a tunnel scheme which received more than ordinary notice. The plan was to form in the Channel thirteen islands by carrying materials out to sea, to sink shafts through them into the earth

below the bed of the Channel, and then tunnel from one to the other. A commission of engineers was appointed, and they carefully examined M. de Gamond's data and conclusions, and particularly his geological investigations, which were the first steps taken to demonstrate practically the nature of the strata beneath the Channel. These investigations supported the theory that the Straits of Dover were not opened by a sudden disruption of the earth at that point, but had been produced naturally and slowly by the gradual washing away of the upper chalk; that the geological formations beneath the Straits remained in the original order of their deposit, and were identical with the formations of the two shores, being, in fact, the continuation of those formations. Acknowledging the scientific accuracy of M. de Gamond's conclusions, the commission recommended an appropriation of 20,000*l.* to make preliminary examinations. The plan, however, was finally rejected on account of its interference with the navigation of the Straits. The submerged tube projectors of that time, with one or two exceptions, appear to have given the subject less study than the advocates of the tunnel. It was a work very easily effected; you only have to construct your tube in sections; float them over the proposed line of route; sink them and connect their ends; pump the water out, and the thing is done. Hear how one of these facile gentlemen describes his project:

"My plan is simply to construct wrought-iron tunnels in separate divisions; to sink them on the bed of the water, and then to connect them. It will be admitted that to construct such a tunnel would be an easier matter than to build iron vessels, as it would be the same shape the whole length. Then to sink it on the bed of the water would be the work of a few hours for each division of 400 ft. in length." A momentary fit of reflection, however, seizes him when he has sunk his lengths of tube, for he says: "Perhaps the part of the work which will appear the most complicated will be to connect the divisions under water." But, quickly recovering himself, he adds: "The operation will be attended with no extraordinay difficulty to those who can remain during half an hour in deep water." Turning to the shore ends he observes: "As regards that part of the tunnel which would be near the shore, it would be sunk under ground, and covered with stones fastened together, so as to render them immovable. . . . . Then the railway will be formed in forming the tube; there will be no hills to cut through, valleys to fill up, nor arches to build; in short, the sum total of the work is comprised in the tunnel itself." Warming with his subject he grows bolder and says: "Supposing the divisons to be 1000 ft. in length; in that case, only 104 divisions would be

required to join the rails of the South-Eastern Railway with those of the Calais and Paris. Now, supposing each of these divisions would cost 40,000*l.*, the cost of the whole would be 4,160,000*l.*; and if we allow for the expense of throwing them in deep water, of connecting them, of building stations, &c., on a magnificent scale, it will, I believe, be found that the sum of 8,000,000*l.* sterling would be quite sufficient to complete the submarine railway." Really the manner in which this gentleman speaks of throwing into deep water iron tubes a thousand feet in length, and capable of containing a railway, is most refreshing, and must have been very encouraging to the sea-sick continental traveller.

Turning to another project we find it to consist in crossing the English Channel by means of a tube made of strong plate iron, or cast iron, lined and prepared for that purpose, and which, placed at the bottom of the sea, should contain the two lines for the trains which would run within it. The slope given to the submarine railway would admit of a motion sufficiently powerful to enable the carriages to cross the Channel without a steam engine. The greatest depth of the sea at the middle of the Channel will admit of the construction of inclined planes, by means of which the train would be enabled to reach a point where a stationary engine, or atmospheric pressure, might be employed in propelling the train to the level of the land railways of France and England.

Another visionary does not place his tube on the bottom, but proposes to situate it at a uniform depth from the surface, by means of ties below (and buoys above, if necessary), at suitable intervals. He says: "The continuation of the tunnel into the shore on either coast I should dispense with; and in order that it should have a partial freedom of motion, it should terminate with solid ends before reaching the shores. To these points, chain piers should extend; or, if strict economy were aimed at in this item, the communication might be by small steamers." As the tunnel or tube in question contains only a single line, the projector proposes pushing one way, and pulling the other; or, he says, electricity should, if at all practicable, be the motive power. After proposing, as the principle of construction, something analogous to the cooper's craft, he continues: "When the tube was completed from end to end, favourable weather would be waited for, and the work of lowering would then be accomplished, nearly as follows: The air-tight interior of each pontoon would be connected by an ample length of flexible pipe to an air-pump of adequate power, on board a vessel anchored at a distance corresponding to that intended for the mooring weights. The two lines of vessels should be manned by steady men, each

crew under a trustworthy leader. Athwart the vessels would be laid, from the shore, the wires of an electric telegraph, communicating with an apparatus on board of each; so that, at a preconcerted signal, the abstraction of the air from the pontoons should be commenced simultaneously at a given time, and carried on at a given rate. By this means, the pontoons, gradually deprived of their buoyancy, would yield to the pressure of their burden—the buoyancy acquired by which as it entered its future element would be overcome by the weights with which, throughout its length, it was loaded, which would speedily sink it to its prescribed depth. It would be when the tube reached the water that the mooring weights, having been preparatorily slung under the vessels above mentioned, would, at another signal by the electric telegraph, be simultaneously let drop into the sea, and drag down their charge along with them."

We will only briefly notice two more of these easy-going gentlemen, whose ideas slip out so glibly that practical men fail to grasp them, and these are the proposer of the arched roadway or tunnel on the bottom, and the proposer of the bridge. The former, with forty subaqueous boats, of which he was the inventor, 1500 sailors and navvies, 4,340,000 cubic yards of material, and 10,000,000*l.*, undertook to construct a tunnel, by means of which the Straits could be crossed in thirty-three minutes. The mammoth bridge projector would make in the Channel 190 pedestals, 300 ft. square at the bottom, consisting of rocks bolted and lashed together, gradually rising at an angle of 75°, till they formed each an insular plain, 150 ft. square, 40 ft. above the level of the sea. On these he would build towers 100 ft. diameter, 260 ft. high; crowning the whole with a tubular bridge 50 ft. deep and 30 ft. wide.

Chalmers.

We will now turn to the more practical projects which have since been advanced, noting them somewhat in the order of their appearance before the public. First, then, comes the proposition for a Channel railway, by the late Mr. James Chalmers, whose name is connected with some very valuable improvements in the construction of armour-plated ships and forts, but which his premature death prevented him perfectly developing. Mr. Chalmers' plan was to have a tube of boiler-plate iron, lined with brickwork, laid on the bed of the sea, and having ventilating towers at intervals. Through this tube the railway was to be carried. His project provided for an unbroken double line, connecting the railways of England and France by easy gradients, capable of carrying all ordinary trains at the usual speeds on the best roads, *and of insuring* perfect safety and comfort. It was to offer no

obstruction to the navigation of the Channel, and Mr. Chalmers estimated that the work could be completed in three years for 12,000,000*l.* The principal features of the work, as proposed in 1866, were two strong iron tubes, cased with timber, and lined with brick, each containing a single line of railway, and reaching from shore to shore on the bottom of the Channel. The displacement and weight of these tubes was designed to be so nearly balanced that both in submerging and when in position they would not be subjected to any appreciable lateral strain. There would be a slight excess of displacement, which would be effectually overcome by the materials with which the tubes were to be covered. Mr. Chalmers held that as the current alternated up and down Channel with the rise and fall of the tide, the embankment would silt up eventually, and become a solid, impermeable mass, having the appearance of a ridge reaching from shore to shore, about 150 ft. wide at the base, 40 ft. high, and from 40 ft. to 120 ft. below the level of low water. Mr. Chalmers proposed to have three ventilators, one in mid-channel, and one about a mile from either shore. Thus the main portion of the work would be eighteen miles in length; and this divided by the deep-sea ventilator, would give two sections of nine miles each. Consequently, a train could never be more than four and a half miles from an opening. From those and other points the air was to be withdrawn through pipes by machinery situated in the central ventilator, or on the shore embankments, which would cause currents of fresh air to rush to those parts most distant from the ventilators.

The tubes were to be circular in form, and made of boiler plate, double-riveted and caulked. The circular form was to be preserved, and the tubes were to be strengthened by iron girder frames surrounding them. To the outer flanges of these frames the timber casing would be attached by bolts; and the spaces between the timber casing and the tube proper filled with concrete. Finally, the interior would be lined with the most durable description of brickwork.

The timber casing, properly caulked, says Mr. Chalmers, would be equal in strength and water-tightness to the plating of a frigate; the tube itself would be as tight and stronger than a steam boiler or iron ship; and the concrete packing between the planking and the tube would also be impermeable to water. Thus, not only is a double or treble precaution taken against leakage, but the iron would be protected, both without and within, from any injurious effects that might result from contact with sea water; though, at the depth at which these tubes would placed, such contact, even if possible, under the circumstances, would not be so injurious as if they were nearer the surface.

Maury, in his "Physical Geography of the Ocean," states as follows:—"Count Marsigli divides sea water into surface and deep sea-water; because, when he makes salt from surface water (not more than 6 in. below the upper strata), this salt will give a red colour to blue paper; whereas the salt from deep-sea water will not alter the colours at all. The blue paper can only change its colour by the action of an acid. The reason why this acid (iodine?) is found in surface and not in deep-sea water is that it is derived from the air." Hence, argues Mr. Chalmers, the bottoms of iron ships—when the interior skin of the iron is kept clean and well-painted—appear to suffer little or nothing from the action of the water on the exterior. But, even if the iron of the tube were to suffer by oxidation in forty or fifty years (a circumstance which, from its position, Mr. Chalmers considers is far from probable) the embankment, by this time silted up into a solid mass, the timber casing, the concrete and interior lining, would of themselves insure the permanency of the Channel railway.

The ventilator in mid-channel was to be a circular mass of iron and stone, 100 ft. in diameter, and 210 ft. in height, 168 ft. of which would be below the water-line. When finished it was to weigh about 100,000 tons, and displace 50,000 tons of water. It was to be surrounded by, and embedded in, the embankment that covered the tubes, which at that point was to be raised to a height of about 80 ft. The other ventilators were to be ordinary air-shafts near the end of the shore embankments, which would be run out, breakwater fashion, about a mile from either shore to a depth sufficient for navigation over the tubes. The tubes were to be made in lengths of 300 ft. or 400 ft., were to be fitted with temporary bulkheads, lowered into position, and united by a somewhat delicate submarine operation.

Hawkshaw.

Mr. Hawkshaw's attention has been directed to the subject of a tunnel for connecting the railway systems of England and France, and he has made a practical geological examination of the Channel and the two coasts, the results of which are of great value in connexion with the question. As Mr. Hawkshaw's scheme appears to have absorbed those of M. de Gamond and Mr. Low, an English mining engineer, it will be as well to trace the history of the combination concurrently with the development of the scheme. The project has been worked out under the auspices of a committee by Messrs. John Hawkshaw, James Brunlees, and William Low, English engineers, in conjunction with MM. Paulin Talabot, Michel Chevalier, and Thomé de Gamond. These gentlemen have reported to the committee of

promoters of the undertaking, and that document supplies the following facts and valuable information, from which we gather the history of the scheme.

Following the order of the report, we will first take Mr. Low's proposition. That gentleman has for several years past conceived the idea of connecting the railway systems of France and England by tunnelling, and as a practical mining engineer he has devoted his attention in the first place to securing the efficient ventilation of the work both during construction and after completion. In most of the tunnel schemes it is proposed to effect ventilation by means of towers in the sea, more or less numerous, and differing in magnitude and cost. Mr. Low proposes to dispense entirely with shafts in the sea, and to commence the work by sinking pits on each shore, driving thence, in the first place, two small parallel driftways or galleries from each country, connected at intervals by transverse driftways. By this means the air could be made to circulate as in ordinary coal mines, and the ventilation be kept perfect at the face of the workings. Another advantage attending this mode of proceeding is that these headings could be driven from shore to shore at the minimum of cost, and the practicability of executing the proposed tunnels demonstrated without extravagant outlay. The driftways were to be turned into two tunnels suitable for the ordinary locomotive traffic of the railways to be connected by this work. Having settled the principle of the construction of his tunnels, Mr. Low investigated the geological nature of the shores of the Straits. From personal examination of the most careful character, he verified the data of Mr. Phillips and of other eminent geologists who have made the geology of the shores of the Straits, and the subject of the continuity of the strata under the sea, their special study. He also examined the borings for the artesian well at Calais, the artesian well at Harwich, and several other wells of less magnitude, by all which the regularity of the strata was proved. He found everywhere that the deductions of the geologists were sound; and at the line which he ultimately fixed on for his proposed tunnels, viz., about half a mile west of the high light of the South Foreland, and at four miles west of Calais, the tunnels could be made almost entirely through the lower or grey chalk, which, owing to its comparative freedom from water, and the general absence of cracks and fissures in it, offered the most desirable stratum for working in. Mr. Low laid his plans before the Emperor of the French in April, 1867, and his majesty desired Mr. Low to proceed to organise the means of carrying out his project, and to come to his majesty again when he was prepared to lay definite proposals before him.

In an international work of this nature, it was desirable to obtain the co-operation of a French engineer, and Mr. Low therefore put himself in communication with M. de Gamond, who placed his geological studies and sections at Mr. Low's disposal. Mr. Low then laid the results of his and M. de Gamond's labours before Mr. Brunlees, who, after a careful examination of the project, consented to co-operate with Mr. Low and M. Thomé de Gamond for the prosecution of the proposed work. In accordance with the desire of his majesty, a committee of French and English gentlemen was formed in furtherance of the project.

For some years past Mr. Hawkshaw's attention had been directed to this subject, and ultimately he was led to test the question, and to ascertain by elaborate investigations whether a submarine tunnel to unite the railways of Great Britain with those of France and the continent of Europe were practicable. With this object he caused to be made a careful examination of the geology of the Channel and of the French and English coasts, and had a chart prepared, based on that examination, and on such further information as he could procure. From a careful consideration of these geological investigations, Mr. Hawkshaw arrived at the conclusion that it was desirable that the tunnel should pass as far as possible through the lower chalk, for it appeared to him that if an attempt were made to carry it through the strata lying under the chalk, it would have to penetrate material of variable nature, some of which would be soft, and through which it would be undesirable to construct a submarine tunnel. It also seemed to him desirable to depress the tunnel as much as possible below the upper or white chalk, and to carry it as far as practicable through the lower or grey chalk, which is less permeable to water than the upper or white chalk. But when he had proceeded thus far, Mr. Hawkshaw felt that more accurate information than could be arrived at by geological inquiries only, however carefully conducted, was necessary. He had ascertained that at Calais an artesian well had been sunk to a depth of about 1000 ft., and from the records of this work he obtained the particulars of the strata at that point. This well, which failed to procure water, was some distance from the spot on the French coast where, from the geological inquiries, it seemed desirable that the submarine tunnel should be placed. He therefore decided to make borings on each coast, at the ends of the line which approximately seemed the best position for the l, and also to examine the bottom of the Channel for some ice on each side of that line. Accordingly, at the beginning e year 1866, a boring was commenced at St. Margaret's near the South Foreland; and in March, 1866, another

boring was commenced on the French coast, at a point about three miles westward of Calais; and simultaneously with these borings an examination was carried on of that portion of the bottom of the Channel lying between the chalk cliffs on each shore.

The boring on the English coast was satisfactorily completed in 1867. It was carried through the chalk and into the green sand, which was reached at a depth of 540 ft. below high water. The boring on the French coast was continued from the surface to a point about 520 ft. below high water. It passed through the upper chalk into the lower or grey chalk. This was completed at the end of 1867. It was Mr. Hawkshaw's intention to have carried this boring also entirely through the chalk, but in attempting to substitute larger boring tubes, the hole was accidentally filled up with sand and shingle from the top. The results, however, arrived at from this boring seem sufficient, and accorded very nearly with the records of the Calais well, and with the geological survey previously made; so as not only to confirm their accuracy, but to lead to the deduction that at the site of this boring the chalk would extend to, and the green sand be reached at, a depth of about 750 ft. below high water.

For the examination of the bottom of the Channel a steamer was engaged and suitable apparatus provided, by means of which the bottom could be pierced for a short distance and specimens could be raised from the bed of the Channel. On shore suitable points can be selected for examining the strata; whilst at sea many of the examinations made from soundings fall on superficial deposit. A survey of the Channel, therefore, cannot be so complete as a survey of the coasts. Nevertheless, the result of the examination seems to indicate that across the Channel the position of the chalk is nearly identical with that deduced from the previous geological inquiries.

The principal practical and useful results that the borings have determined are, that on the proposed line of the tunnel the depth of the chalk on the English coast is 470 ft. below high water, consisting of 175 ft. of upper or white chalk and 295 ft. of lower or grey chalk; and that on the French coast the depth of the chalk is 750 ft. below high water, consisting of 270 ft. of upper or white chalk and 480 ft. of lower or grey chalk; and that the position of the chalk on the bed of the Channel, ascertained from the examination, nearly corresponds with that which the geological inquiry elicited. It also appears probable that there is no great fault or serious interruption in the continuity or regularity of the strata between the two shores on the proposed line of tunnel. The results of these investigations were submitted to M. Michel Chevalier and M. Paulin Talabot, and

they were put in possession of all the information that had been obtained.

Such, then, is the history of the separate investigations which have been made into this important and interesting subject, and the general nature and result of those investigations. We will now proceed to the conclusions at which the members of the Committee of Engineers arrived after consulting together and comparing the several data.

They submit that it is evident that at some sufficient depth below the bottom of the Channel a tunnel could be constructed, so that, as regards superincumbent pressure only, it would be analogous to constructing a tunnel of similar length through a mountain so high as to prohibit intermediate shafts; it is further evident that any possible irruption of sea water may be avoided by going deep enough below the bottom of the Channel. On the other hand, they observe that there is a limit to the depth at which the tunnel can be carried, from the necessity of approaching it from the shore and obtaining gradients for those approaches suitable for railway traffic. If the tunnel were carried through the upper or white chalk, or chalk with flints, apart from a possible irruption or percolation of water from the sea, fresh water might be encountered to the usual extent that it is met with in that formation. But the tunnel can be depressed so as to pass mainly through the lower or grey chalk, which is less permeable, and where the quantity of fresh water would be comparatively unimportant, provided no great fault or dislocation of the strata exists — and the investigations lead to the conclusion that no such great fault does exist. It is probable that at the depth below the bed of the Channel at which it would alone be prudent to carry the tunnel, any fissures that may occur in the chalk have been filled up, and that from this circumstance and from the nature of the lower chalk no more water will be met with than can be overcome by pumping.

With regard to the execution of the work itself, the Committee of Engineers consider it proper to drive preliminary driftways or headings under the Channel, the ventilation of which would be accomplished by some of the usual modes adopted in the best coal mines. All other questions in relation to the construction of the permanent tunnels would be decided from the experience gained in making the driftways, and it might even be deemed advisable to commence the formation of the permanent tunnels before the completion of the driftways, if circumstances indicated the desirability of that course. They propose that the tunnel should be of the ordinary form, sufficiently large for two lines of railway, and to admit of being worked by locomotive engines, and artificial ventilation could be

applied. The desirability of adopting other modes of traction is left for future consideration.

Finally, the following general conclusions are submitted:

1. That there is a reasonable prospect that the work can be accomplished, but that it would be improper to deny that it is attended with a certain amount of risk.

2. That this risk is limited to one contingency—viz., the possibility of sea water finding its way by some unforeseen fissure into the workings in quantity too great to be overcome. Apart from this risk, tunnelling in chalk is easy and rapid, and the execution of a tunnel of the length of the one under consideration is only a question of time and expense.

3. There seems to be no reason to assume that the tunnel would cost more than ten millions sterling, or that it could not be completed in nine or ten years.

4. The question of risk would be fully solved by sinking land shafts on each coast, and driving the preliminary driftways. This portion of the work being safely accomplished, the remainder would be of an ordinary character.

5. The possible loss would be measured by the cost of this preliminary work, which is estimated at one million and a half, and which could not exceed two millions, or (say) one-fifth of the whole cost of the tunnel.

6. That this risk should be undertaken by the Governments of France and England, if, after consideration, they deem the importance of the work and the probability of its completion sufficient to justify them in doing so.

Austin.

Towards the close of last year Mr. W. Austin proposed a submarine three-way tunnel under the Channel. His plan is to cross at a line of route extending from the landing piers at Folkestone to the landing piers of Cape Grisnez, but the tunnel will range below the sea bed, at a safe depth for practical permanent masonry arches, which will be constructed of imperishable materials, on an improved principle of vertebraical bond. The tunnel is intended to pass underneath the submerged island, called the Varne, lying near the mid-channel, and on which island Mr. Austin proposes to erect a central ventilating shaft or tower, which would be available as a permanent central lighthouse and naval signal shaft; also to afford a refuge or retreat for crews of ships wrecked in the Channel. As a fence or guard to this central tower or shaft, it is proposed to have two ranges of timber floating breakwaters, so as to act as floating retreats for ships, and protectors to the tower shaft from hurricanes or gales. Two other masonry shafts will be permanently constructed for ventilation and

pumping purposes at each shore. Seven or eight temporary shafts will also be constructed in iron, and sunk and bored down to the tunnel arching, so as to give ventilation to workers in construction of the tunnels, and also to remove a portion of the excavated *débris*. These temporary shafts would be protected by moored floating booms or fences during the construction of the tunnel, and, on completion of the tunnel, the temporary shafts and booms would all be removed, having done their work. The gradients of inclines of the proposed tunnels are so arranged that the steepest gradients of the two shore inclines, or connexions with the main land railways, in England and France, do not exceed 1 in 100, so that locomotives of moderate powers would accomplish the required work easily. Occasional openings are to be constructed in the masonry range of tunnel wall sides, so as to allow for a traverse of engines and carriages from one range of tunnel to another, in the event of any accident or emergency, when traverse frames would quickly shift the disabled carriages out of the way of an obstructed traffic.

Arrangements of a distinct and peculiar character were to be made for the proper ventilation of the tunnels, by air and water streams; also for the lighting the tunnels throughout, by perfected modes of gas burning, in specially constructed lamps, &c. Every facility will be provided for laying down a perfect system of telegraph conducting wires, which will be easily accessible for adjustment and repair; and the present great risks and accidents, now so often occurring, of tearing up telegraph cables by ship's anchors, will then be avoided. Subways are to be constructed throughout the tunnel ranges, which will exhaust any accumulation of steam and waste waters, or temporary leakages, and which water will be passed through well-pits, and then ejected by pumps, connected with the great central shaft and two shore shafts. The advantages of three tunnel ranges will be to keep special, ordinary, and goods traffic trains separate and distinct, and thus obviate present causes of frequent accidents by clashing trains conveying passengers and goods on the same ranges. Mr. Austin states that his scheme would cost 17,600,000*l.*

Fowler.

Mr. John Fowler has for several years past directed his attention to the best mode of improving continental communication, and he prefers a system of ferrying the trains over in steam vessels rather than either a bridge or a tunnel. He has for the last two years associated with him Mr. Abernethy, and Mr. Wilson as acting engineer. After a careful examination of the harbours of Calais and Boulogne and the coast between them,

and after considering the general question, these gentlemen came to the conclusion that the following were essential requirements:

1. Well-sheltered harbours with deep water on both coasts capable of affording ingress and egress at all times, irrespective of weather and tide.

2. A class of vessels, in the nature of ferry-steamers, of greater size and power, making rapid passages, and comparatively unaffected by wind and sea.

3. Safe and speedy means for the interchange of passenger traffic between the railways and such ferry-steamers, so as practically to form a continuous communication.

Conceiving that the harbour and pier at Dover afforded very inadequate terminal accommodation, and insufficient protection, particularly from easterly and south-easterly gales, they propose the construction of a new harbour westward of the existing pier, with a graving dock, a covered berth for the steamers, and hydraulic apparatus for transferring the trains from the quay to the steamers, and *vice versâ*. They consider that Calais, which was exposed to all winds from west to east, and was choked up with sand, offers no facility for a harbour for such steamers; and that the harbour of Boulogne, though more readily capable of partial improvement, partakes of the same natural defect of sand accumulation. They have, therefore, selected a point of the coast south of Cape Grisnez and north of Ambleteuse, near the village of Audresselles, where the deepest water was to be found near the coast, and sheltered from the northward and eastward by Cape Grisnez. They consider that, by constructing a harbour at this point, they will secure the shortest available sea passage, with clear navigation across the Channel from Dover, and with the advantage of the first-class light on Cape Grisnez as a guide on the darkest nights. They propose to connect this harbour by a railway four miles long with the Chemin de Fer du Nord, and to add a short branch for communication with Calais and the north.

The steam vessels the projectors of this scheme propose to employ between these harbours are to be 450ft. long, with 57ft. of beam, and 80ft. across the paddle-boxes, propelled by disconnected engines of 1500 horse power, performing the voyage in one hour, and with comparatively little pitching or rolling in any state of the weather. They further propose to form covered stations for the interchange of passenger and goods traffic between the steamers and the shore, the transfer of the passenger carriages to be effected by hydraulic apparatus, irrespective of tide, in a few minutes. The steamers are to be luxuriously fitted up with first and second-class refreshment saloons, and with ladies' and private cabins; and, in addition to the saving

of time in the sea passage, and on either coast, the passengers are to have the advantage of being able to secure and retain the seats in their railway carriages throughout the journey between London and Paris and other centres. At the same time the various saloons and cabins and the decks of the steamers will be at their service. The projectors believe that the extra comfort to the passengers afforded by such accommodation, and by the regularity of the service, is far more important than the saving of time that would be effected. They reject as insufficient and unsuited to the requirements of the present day, all partial improvements of Boulogne Harbour and the service between that harbour and Dover or Folkestone; and they estimate that these works, and the railway steam-ferry steamers, may be completed within three years, at an expenditure of 2,000,000*l.*

Remington.

In 1865 Mr. George Remington published a plan for the construction of a tunnel railway from Dungeness to Cape Grisnez. Before advancing his scheme Mr. Remington ascertained as far as possible the nature of the geological strata forming the bed of the Channel. He found that the coast in the neighbourhood of Dover and Folkestone consisted of chalk, which extends under the Channel to the Calais shore. As Mr. Remington concluded that this formation, which is in itself so very porous and contains so many fissures, could not be suited for tunnelling under a great head of water, he visited Dungeness, and examined the whole line of shore from Hastings to Dover, and satisfied himself that the Wealden formation, consisting of very strong clay, beds of freestone, and freshwater limestone, extended from Dungeness across the Channel to Cape Grisnez. Having concluded that that was the proper course for the construction of the tunnel, he at once proceeded to prepare plans and sections, and laid them before the Board of Trade, the Minister of Works, Paris, and others. This proposition has again been brought before the public within the last few months.

The line is intended to commence at the town of Lydd, where it will join the branch railway from the South-Eastern at Appledore. It will descend from Lydd at an inclination of 1 in 70, the distance of 3½ miles to the point of Dungeness, where the level of the rails will be 240 ft. below the level of low-water spring-tides. The rails would then rise from Dungeness shaft at the rate of 1 in 3795 for about 7 miles, and then fall at the rate of 1 in 1200 for about 8 miles, to the centre shaft on the "Ridge;" from thence fall at the rate of 1 in 3265 for 11 miles to the Cape Grisnez, and then rise at the rate of 1 in 70 and 1 in 81 to join the French railways.

The height of the tunnel will be 30 ft. from the soffit of the arch to the centre of the invert, and there will be a clear headway of 20 ft. for the trains; the space between the rails and the invert will be occupied by a sewer, running along the centre line of the tunnel, and on each side of it two air tunnels, for the purpose of providing ventilation. The width of the tunnel will be 25 ft.; it will be constructed of brickwork and masonry, surrounded with concrete, and also a mass of concrete will be placed upon the invert surrounding the air and drainage tunnels, and forming a bedding for the sleepers of the railway.

There will be three main shafts of large dimensions. The centre shaft on the "Ridge" will be protected by a breakwater, formed of rubble and faced with ashlar. The other shafts are to be effected by means of wrought-iron tubular piles from 8 ft. to 10 ft. in diameter, the inside strengthened with plates on the cellular principle. These piles will be provided with valves to regulate the ingress and egress of water during the time of sinking into position, and when sunk they will be supported by proper guy chains and tackle from anchor moorings placed in various directions around the piles, every pile forming a shaft of sufficient length to reach the entire depth of water and through the bed of the Channel down to the level of the tunnel. It is intended to weight the lower end of the piles, and to sink them into position on the principle of the angler's float. The water will then be pumped out by steam power, and the soil be brought up from the interior and cast over on the outside, forming a cone round the pile. Mr. Remington estimates the cost of these works at 6,998,200*l.*

Marsden.

Mr. Charles Marsden's scheme, which was brought before public notice in April last, consists of a tubular tunnel, made of boiler plate, and having a double skin, on the well-known principle used in ship-building, the annular space being filled in with artificial stone. Mr. Marsden proposes to carry pipes for ventilating the tunnel, as well as pipes for water, sewage, and telegraph wires, at the top of the tunnel. At the bottom runs the line of road, on which the railway is laid. The ventilating pipes are formed with longitudinal slots. Air is to be forced from the shore end into these tubes, which will pass through the slots into the tunnel, and ventilate it. Mr. Marsden has a special plan for jointing the several lengths of the tube. It consists in fitting one end of the tube with a series of stepped or curved plates, which are secured in place by screws or rivets. The adjoining end of the next pipe is fitted with a bulge or socket. Another series of plates is employed, one end of which is forced under

the open end of the stepped plates. They are prevented from falling out by being guided into position by a number of rings, fitted on the end of the pipe. Another ring is employed on the outside, to hold the plates. A portion of this ring is looped or pocketed, to enable the plates to be placed in position. When passed through this looped portion they are made to slide round in succession, so as to meet each other, until the whole diameter of the pipe end is covered. They are then firmly secured in place by keys being driven in between them and the ring. The loop is then filled in with a wedge-piece, and thus the whole joint is made sound, flexible, and tight. The joint being flexible admits of its adapting itself to the general contour of the bed of the Channel, and following any undulation caused by a settlement of the soil.

It is proposed to lay this tunnel in the following manner:—A cutting is first to be made on land about two or three miles in length, gradually descending to a point where the water is about ten fathoms deep. A sea wall is to be built about 50 ft. from the water's edge. A portion of a cylinder is to be built in the wall and made water-tight, and the sea is then to be allowed to flow up to the wall, so that the next cylinder can be floated and placed in a proper line. The same with the next, and so on, lowering them from a raft which they will form. Mr. Marsden proposes that divers should cut through moderately high projections in the Channel bed, and also the levels, making a trough, into which would be lowered clay, the divers puddling the same, and thus forming a bed for tubes to rest upon. The cylinders can be made on the coast, and, when finished, have their ends stopped and be towed to the spot required, half filled with the artificial stone composition. Four or six of the tubes can be used to form a raft or stage to carry the machinery for lowering and fixing the tube in place, and afterwards can be used on the land end to finish the connexion on shore. By this means Mr. Marsden estimates a working tunnel could be completed in about four years, without the danger of its being flooded. The heading at each end is to be made in three or more parts, and well secured with india-rubber, or other packing, and screw bolts. The heading, after two or three cylinders are fixed together from the outside, is to be removed from the inside, making an entrance from the shore end into the next tube, and so on throughout. The double cylinder is braced to keep the inner from the outer. The space between the two, which is 5 ft. or 6 ft., is to be filled with tarred granite and asphalte, which will form a perfect wall. As Mr. Marsden considers it impossible to have a ventilating shaft in the Channel, he proposes to have machinery on each shore to abstract the vapours from,

and also to force air into, the tunnel. Mr. Marsden's estimate for a tunnel between England and France on his principle is 12,260,000*l.*

Bateman.

The uncertainty of the strata in the bed of the Channel and the risks of tunnelling under the sea have led Mr. J. F. Bateman, in conjunction with M. Julian J. Rèvy, an Austrian engineer, to propose a cast-iron tube for carrying a railway across the Channel. The distance to be crossed, and the cost to be incurred, undoubtedly require that the mode to be adopted shall be absolutely free from serious doubt and risk, and shall be as evidently capable of accomplishment as the most ordinary mechanical operation. Some degree of uncertainty must exist in every contrivance and speculation; but unless a scheme can be proposed which will be free from all doubt and objection, so far as human knowledge and foresight can extend, it will hardly deserve, and will probably not receive, the support of the public. The various proposals for constructing submarine tubes have been carefully studied by Mr. Bateman and M. Rèvy, who have come to the conclusion that none of them are free from serious objections, both on the score of difficulty of construction and the dangers which would attend the operations as proposed. Their object, therefore, has been to devise a scheme by which all difficulties of operating in water should be avoided.

They propose to lay a tube of cast iron on the bottom of the sea, between coast and coast, to be commenced on one side of the Channel, and to be built up within the inside of a horizontal cylinder or chamber, which shall be constantly pushed forward as the building of the tube proceeds. The chamber within which the tube is to be constructed will be about 80 ft. in length, 18 ft. internal diameter, and composed of cast-iron rings 8 in. thick, securely bolted together. The interior of the bell will be bored out to a true cylindrical surface like the inside of a steam cylinder. The tube to be constructed within it will consist of cast-iron plates in segments 4 in. in thickness, connected by flanges, bolted together inside the tube, leaving a clear diameter of 13 ft. when finished. Surrounding this tube, and forming part of it, will be constructed annular discs or diaphragms, the outside circumference of which will accurately fit the interior of the bell. These diaphragms will be furnished with arrangements for making perfectly water-tight joints for the purpose of excluding sea water and securing a dry chamber, within which the various operations for building up the tube, and for pressing forward the bell as each ring of the tube is added, will be performed. There will always be three and generally four of these

water joints contained within the bell. A clear space between the end of the tube and the end or projecting part of the bell of 36 ft. will be left as a chamber for the various operations. Within this chamber powerful hydraulic presses, using the built and completed portion of the tube as a fulcrum, will, as each ring is completed, push forward the bell to a sufficient distance to admit the addition of another ring to the tube. The bell will slide over the water-tight joints described, one of which will be left behind as the bell is projected forward, leaving three always in operation against the sea.

The weight of the bell and the machinery within it will be a little in excess of the weight of water displaced, and, therefore, the only resistance to be overcome by the hydraulic presses when pushing forward the bell is the friction due to the slight difference in weight and the head or column of water pressing upon the sectional area of the bell against its forward motion. In like manner, the specific gravity of the tube will be a little in excess of the weight of water which it displaces; and in order to obtain a firm footing on the bottom of the sea, the tube will be weighted by a lining of brick in cement, and for further protection will be tied to the ground by screw piles, which will pass through stuffing boxes in the bottom of the tube. These piles will, during the construction of the tube within the bell chamber, be introduced in the annular space between the outside of the tube and the inside of the bell, and will be screwed into the ground as they are left behind by the progression of the bell. The hydraulic presses and the other hydraulic machinery, which will be employed for lifting and fixing the various segments of the tube, will be supplied with the power required for working them from accumulators on shore, on Sir William Armstrong's system, and the supply of fresh air required for the sustenance of the workmen employed within the bell and within the tube will be insured also by steam power on shore. As the tube is completed, the rails will be laid within it for the trains or waggons to be employed in bringing up segments of the rings as they may be required for the construction of the tube, and for taking back the waste water from the hydraulic presses, or any water from leakage during the construction.

The tube will be formed of rings of 10 ft. in length, each ring consisting of six segments, all precisely alike, turned and faced at the flanges or joints, and fitted together on shore previously to being taken into the bell, so that on their arrival the segments may, with certainty and precision, be attached to each other. Every detail of construction has been designed, and, so far as the authors of this project can see, no contingency has been left unprovided for. The possibility of injury by anchors

or wrecks, or submarine currents, has also been investigated. The tube when laid will be secured from all dangers arising from such causes.

The building of the tube will be commenced on dry land above the level of the sea, and will be gradually submerged as the tube lengthens. The operations on dry land will be attended with more difficulty than those under water, but all these circumstances have been carefully considered and provided for. The rings forming the tube will be made by special machinery, to be expressly constructed for facilitating the work and economising the cost. This machinery is all designed and specified. The first half-mile will test the feasibility of construction, for that will have to be built both above and under water. When once fairly under water, the progress should be rapid, and it is estimated that the whole undertaking may be easily completed in five years from the commencement.

The precise line to be taken betwixt the English and French coasts can hardly be determined without a more minute survey of the bottom of the Channel than at present exists. It will probably be between a point in close proximity to Dover on the English coast, and a point in close proximity to Cape Grisnez on the French coast. From an examinatiou of the Admiralty charts, and of such information as at present exists, the sea bed on this line appears to be the most uniform and level, and, while free from hard rocks and broken ground, to consist of coarse sand, gravel, and clay. The average depth of water is about 110 ft., the maximum about 200 ft. On the line suggested, the water increases in depth on both sides of the Channel more rapidly than elsewhere, although in no instance will the gradient be more than about 1 in 100. The tube, when completed, will occupy about 16 ft. in depth above the present bottom of the sea. Up to the point on each shore at which the depth of water above the top of the tube would reach (say) 30 ft. at low water, an open pier or other protection would have to be constructed, for the purpose of pointing out its position, and of preventing vessels striking against the tube. These piers may be rendered subservient to harbour improvements. The tube at each end would gradually emerge from the water, and on arriving above the level of the sea would be connected with the existing railway systems, so that the same carriage may travel all the way from London to Paris.

The distance across the Channel on the line chosen is about twenty-two miles. The tube as proposed is large enough for the passage of carriages of the present ordinary construction, and to avoid the objections to the use of locomotives in a tube of so great a length, and the nuisance which would be thereby created,

and taking advantage of the perfect circular form which the mechanical operation of turning, facing, &c., will insure, it is proposed to work the traffic by pneumatic pressure. The air will be exhausted on one side of the train and forced in on the other, and so the required difference of pressure will be given for carrying the train through at any determined speed. Powerful steam-engines, with the necessary apparatus for exhausting and forcing the air into the tube, will be erected on shore at each end; and supposing one tube only to exist, the traffic will be worked alternately in each direction. This system of working the traffic will secure a constant supply of the purest air, which will accompany every train.

The estimated cost of the whole undertaking, including the stations and approaches at each end, the engine power and machinery, the interest of outlay during construction, and engineering superintendence, with a large margin for contingencies, is 8,000,000*l.*

GRANTHAM.

Mr. John Grantham's proposition for improving the communication between England and France is to turn the present system to account, and to make an efficient means of transit whilst the more elaborate schemes are being perfected and rendered practical. Mr. Grantham proposes steam-ships 400 ft. long, 45 ft. beam, and 600 nominal horse power, to be built of steel, and to draw 6 ft. 6 in. water, and to steer from either end, to avoid the necessity of turning. The lower part of the ship is to be cellular, so as to avoid the danger of sinking if injured by a collision, and to add to her strength. This cellular portion is 6 ft. in depth; over it would be the main saloons, these to be 10 ft. in height, and provided with ample accommodation for reclining in bad weather; good refreshment tables also would be provided with light and air to avoid that gloom and closeness so painful to landsmen when on board ship. Between the paddle-boxes would be a promenade deck 100 ft. long, on the extremities of which would be the houses from which the vessel would be steered. To strengthen her amidships, with the least amount of weight, there would be provided four open longitudinal girders, about 250 ft. long, so arranged as to cause very little obstruction on the ship, and longitudinal bulkheads, about 90 ft. long, would run through the engine compartment. In addition to these precautions, the plates of the sides would be extended up, and form the inner side of the [illegible]box, drawn off towards the ends at the same angle as the [illegible]girders.

[illegible]ler to facilitate the transfer of passengers and luggage

from the piers to the ship, wide and ample means are provided, at three different elevations, to receive the stages from the shore. The lowest of these is on the level of the main deck; the next is on the level of the promenade deck, and the upper one is on the paddle-boxes or on the captain's platform; so that, whatever should be the state of the tide, the stages may rest on the top of the piers, and be nearly level. On the piers are to be erected sheds, under which the trains are to run, as in ordinary railway stations, to be well inclosed and lighted, so that but little delay or discomfort would arise. The staircases leading from the promenade platform to the deck should be easy, wide, well-lighted, and sheltered. The great bulk of the luggage would be stowed in trucks, the bodies of which would be hoisted by hydraulic cranes on to the deck, and again lifted off on the other side, and placed on wheels provided for them. In all this there is no novelty introduced; all that is proposed may be seen in operation in other places. The only question that can be raised is as to the facility of working a vessel, such as is here described, in and out of the harbours. For experience in this, Mr. Grantham refers to the American steamers, which are managed with wonderful precision. These vessels have, in some instances, exceeded 400 ft. in length. Those on the rivers draw 5 ft. and 6 ft. of water, and those which put to sea vary from 7 ft. to 10 ft.; but all are of immense height out of water, the wheel-houses of some being upwards of 40 ft. above the water-line. Mr. Grantham's vessel, though drawing only 6 ft. 6 in., will not exceed 15 ft. above the water-lines at the ends. Long vessels steer much more steadily in heavy weather than short ones, and will, therefore, enter such harbours as Calais or Boulogne with more safety than the present vessels, and, when fairly entered, there is time to bring them to a standstill before reaching their berths.

Mr. Grantham gives the following reasons for not adopting a vessel in which the trains should be carried bodily across:—First, there is the necessity of placing the vessel in a position of perfect stillness before taking the trains on or off the ship. This involves new harbours on each side, with dock gates and break-waters to protect them. Second, the large ships necessary to carry the trains will take a long time in bad weather before they can be placed in position and the gates closed. Third, very few persons would remain in the carriages when being put on board, and, in the event of sea-sickness, they could not be allowed to remain, so that the passengers must have time to leave the carriages and walk on board. Lastly, the large outlay in forming the harbours, appliances for shipping the trains, and the extra cost of the ships, together with the heavy cost of working them.

Boutet.

The great bridge scheme for connecting the two shores, which is at the present time attracting a large amount of attention, both in England and France—especially in the latter country—is the project of M. Charles Boutet. This gentleman, who is a French engineer, has been engaged during the past ten years in maturing his plans and constructing models in order to satisfy himself of the correctness of the principles he has enunciated. M. Boutet selects a point on the Dover Hills, near the Shakspeare Cliff, for the commencement of his International Bridge, which is to touch the coast of France at Blanc Nez, a short distance from Calais, at which place the cliffs are about the same height as those on the English coast. These cliffs, at either extremity rising nearly 400 ft. above the level of the sea, serve as abutments for the proposed viaduct. To protect them against the destructive action of rain, winds, and frost, they will be faced with a solid construction of dressed stone. The project depends in effect on two remarkable innovations in the construction and establishment of the piers—of which there will be twenty-nine—and girders. In addition to the considerable height to which the former rise above the water (120 yards), the bases of the piers are sunk to the bottom at a depth varying from 28 yards to 52 yards. The centre pier will be half as large again as the others. All the pieces composing the work are of cast iron.

As such ponderous piers could not be erected by the ordinary means, M. Boutet proposes to construct on the shore their lower parts or bases to a height sufficient to rise 10 yards above high water, and as soon as the iron skeleton is put together and bolted, a number of large sheet-iron buoys are distributed about the surface of the base. At low water the metallic framework thus prepared is made to slide upon the shore to low-water mark. The tide in rising raises this raft or base of iron lightened by the buoys, and floats it. A tug steamer then removes it to its place, previously indicated by one of a line of buoys attached to an iron cable stretched across the Straits at a depth of 18 yards. By raising one of the buoys attached to the raft it is made to descend very slowly, the top being just above the level of the sea, when the base touches the bottom. It is foreseen that it may become necessary to rectify its position, and a strong metal screw is therefore placed in the interior of each pier, extending from base to summit, to which is attached a large buoy, which can thus be moved from the top to the bottom, or *vice versâ*. The buoy turbing the equilibrium renders the specific weight of the

pile heavier or lighter than the displaced water, and the whole structure when floated to its place can be sunk without the least shock. It is manœuvred like a ship, and its position is verified by a special glass, invented by M. Boutet, to be placed on the cliffs. The base of the pier is provided with large screws, or spiral feet, which, on being turned, bind it firmly to the solid bed of the sea, and serve to establish the level if necessary. Such is the method of moving and fixing the enormous bases of the piers, the component parts of which it would otherwise be extremely difficult to unite under water. The construction of the upper portion of the pier above the sea is effected piece by piece. Thus are avoided all the preliminary works under water, which constitute the greatest difficulty in the way of a bridge across the Channel.

Next, with regard to the superstructure, M. Boutet has hit upon a plan whereby the difficulties of transport, &c., are overcome. He constructs rigid beams endowed with great powers of resistance of a weight relatively very small, and capable of being placed in position piece by piece, by the aid of a system of scaffolding constructed as follows:—Between the abutment on the shore and the first large pier, three temporary piers are placed at equal distances. This done, there are stretched in parallel lines a number of wire cables 2 metres (*i.e.*, 2 yards 6 in.) apart. They are connected and bound together by ties made of smaller cables, which interlace the large ones and hold each in its place. The whole forms a truss of 63 yards wide. The truss thus made is covered by a wooden flooring, a guard is fixed on each side, and there is at once obtained a service bridge upon which scaffolding is erected to support the roadway of the bridge during its construction, the scaffolding being always at a sufficient height above the sea to allow the largest vessels to pass under it. Under such scaffolding are supported the wire cables forming the roadway of the bridge—each of which is strained as nearly to a right line as possible, after which smaller cables are interwoven, bracing together the main cables and holding them firmly in their places. Each cable is composed of eight iron wires parallel to each other and bound together at intervals with strong wire collars. The straining of the large cables is to be managed by means of weights, which are removed after the proper degree of tension has been attained and the cables fastened down to the tops of the piers. For example, we will assume the cable to be fixed at the abutment and laid loosely over the other piers, the weight would then be applied to the length of cable between the first and second piers. This would bring the length between the abutment and the first pier taut, and it would be made fast to the first pier. The weight would

then be removed to the cable between the second and third piers, and the process would be repeated until every span of cable was stretched to its proper limit. The cables are carried without a break over the tops of the piers; each cable, therefore, will be twenty-one miles long. Above and below these cables are fastened beams of timber, over which the permanent way will be fixed. The author is informed that Mr. Ordish has pronounced that portion of the scheme relating to the piers to be practicable, and the method of floating the bases exceedingly ingenious. Another eminent engineer, the chief of our leading railways, but whose name the author is not at liberty to mention, has examined M. Boutet's plans for the superstructure, and states that the scheme is perfectly feasible. He, however, sees a difficulty about the piers, but, as we have it on the authority of Mr. Ordish that the piers are practicable, we may conclude the whole structure to be so. The bridge is estimated to cost 8,000,000*l.*, and to occupy three years in completing. It is stated that experiments in progress give reason to expect that this estimate will not be exceeded.

The carrying out of M. Boutet's principle by the construction of bridges generally on a smaller scale than would be required for the Channel, appears to be now imminent. The author understands that two small bridges have already been constructed, one for the College at Verviers and one at Senlis. A company has been formed in Paris, and the Emperor has directed a bridge to be constructed across the Seine, the first cable for which is already made. A span of half a mile at St. Malo is to be bridged upon the same principle. It would be premature to discuss the scientific details of M. Boutet's project, as they are not yet made public, and, indeed, cannot be determined until the result of some experiments in Paris, and which are now pending, are made known. The arrangements for these experiments are now nearly completed, and it is proposed that they shall be attended by the principal engineers in France and England, to whom invitations are to be sent. We may, therefore, conclude that its merits and demerits will then be investigated, and the practicability of the scheme thoroughly ventilated.

A preliminary meeting of the Channel Bridge Company was recently held in Paris, when several models of bridges upon M. Boutet's system were exhibited. One model in particular illustrated the principle of the proposed Channel Bridge. It consists of one span, and is constructed upon a scale of 1-50th of the d international bridge, and is formed of 21 small cables, imetres in diameter, bound together at intervals by ties t angles, which have the effect of keeping the larger

cables separate, and at the same time preserving them in their relative positions. The whole forms an open truss, which is bolted between planks of wood, thus securing perfect rigidity, which is further increased by the balustrades. It is reported that the weight of forty persons stamping together on this model failed to produce any sensible deflection. It is estimated that it would require a weight of over 50,000 kilogrammes to break it. This model rests on abutments which have been roughly hewn in logs of somewhat decayed timber; this is a convincing proof that the abutments are not subjected to any strain.

Colburn.

Mr. Zerah Colburn has recently proposed a novel method of facilitating the laying of a tube across the bed of the Channel. He has worked out all the details, but inasmuch as the idea forms the subject of a pending patent, only a general outline of the proposition can be given. He proposes to construct a dry dock on the coast at the point where the tube is to be carried across. This dock is to be of any reasonable length, from a a thousand feet to a mile, and only a few feet wider than the outer diameter of the tube. The sections of the tube are to be united together in this dock, the seaward end being fitted with a water-tight bulkhead and projecting through a water-tight opening in the dock gates. When a length of (say) a thousand feet of tubing was ready, the rear end would be fitted with a bulkhead, the water admitted into the dock, and the tube slowly towed or floated out until the last section reached the dock gates. The gates would then be closed, the water pumped out, and the work proceeded with as before. The specific gravity of the tube is to be such as that it shall just sink to the bottom, and each time it is drawn out it is to be slightly raised off the bottom by a system of buoyage, which is also to assist the flotation of the tube as it progresses in its gradual passage across the Channel. On reaching the opposite shore, the tube, after being made secure, is to be lined with brick in cement, and rails laid, and in other respects the work is to be completed.

Parsons.

A pontoon vessel, of shallow draft, for the Channel passage, has recently been proposed by Mr. J. H. Parsons. He connects four pontoons together by cross girders, leaving a waterway between the pontoons for the paddles, of which there are to be six—three forward and three aft. The boilers, coal bunkers, &c., are to be placed in the centre of the vessel, and the engines at each end. In the deck arrangements, the idea is to have a

sleeping and a general saloon, separate from each other. A clear passage is to be left from stem to stern for working the vessel and for a promenade for passengers. This vessel, it is contended, would be rendered independent of the tide in crossing the bar at Boulogne, and would make the service direct, instead of tidal, as at present. The author believes that Mr. W. Bridges Adams has also proposed a somewhat similar vessel to that of Mr. Parsons.

Waring.

The present paper would be incomplete—as far as the author's knowledge extends—were he to omit to mention a proposition by Messrs. Waring to effect the Channel passage. But what that proposition is the author cannot say, as, on inquiry at Messrs. Waring's office, the representative of that firm could only say that they "had a concession." What that concession was for, whether for a tunnel, a tube, a ferry, an embankment, a bridge, or an overhead railway, the author cannot say, inasmuch as, to the author's inquiries on this point, the gentleman alluded to was specially reticent and painfully mysterious. The only subsequent light thrown upon the subject is a paragraph in a Dover paper to the following effect :—"We hear from good authority that a 'committee for the defence of the Channel ports' has been formed at Calais, Boulogne, and Havre, to oppose the project attributed to Messrs. Waring, the English contractors, of constructing a more convenient port near Cape Grisnez, which is certainly a very illiberal piece of obstructiveness." That the scheme is one of larger vessels and increased port accommodation appears from a statement of the Paris correspondent of the *Morning Post* of the 12th inst., and who writes thus: "We are again talking about a bridge over the Channel; the subject is sure to crop up periodically. If it is not a bridge it is a tunnel, and whether one or the other, or the proposed comfortable large boats of the Messrs. Waring, every traveller, even the non-seasick, wish the project success."

As an embankment has been proposed as a means of facilitating the means of communication between London and the Continent, it may here be briefly referred to. An embankment, however, appears to be about the worst possible solution to the problem, firstly, because of the difficulty of carrying it out, and, secondly, because of the tremendous loss of property that it would cause. In other words, it would be very difficult to find the pecuniary means, and the project would encounter the most formidable opposition from the neighbouring populations of the F[illegible]nd English coasts. This would arise from the damage [illegible]kment would cause to the fishing populations of Rams-

gate, Deal, Dover, Calais, Boulogne, &c. Besides, the proposer of this scheme would be puzzled to find the necessary amount of soil, making a liberal allowance for the slopes, which might be washed away a dozen times before they could settle down and acquire cohesion and stability.

Mr. W. H. Barlow has proposed a submerged bridge, but the particulars of this scheme have not come under the author's notice.

Mr. Charles Boyd proposes a marine viaduct from Dover to Cape Grisnez, constructed with iron girders on 190 towers, 500 ft. apart, and 500 ft. above the sea, and he estimates the cost of such a bridge at 30,000,000*l.*

Mr. Hawkins Simpson has addressed the Board of Trade on the subject of working a submarine tunnel on a pneumatic system, which he has termed his "Eolian system," for which he claims cheapness, expedition, superior ventilation, and greater utility.

Mr. Alexander Vacherot has a scheme, on which he has been engaged several years, and which he laid before the Emperor of the French in 1856, for laying on the bed of the sea a tunnel made or formed of concrete, so as to form, when completed, a monolith. He would construct it on the shore, and draw it down to its place in sections. And he considers that greater economy and security might thus be obtained than by the other methods that have been proposed.

Estimates.

It is of course taken for granted that the adoption of either tunnel, tube, or bridge, would prove a success commercially. But as this is an important point, and one upon the proof of which any project must be dependent, it may be as well to state a few facts in support of the argument that such an undertaking will pay. It, however, needs not that very much be said upon this point, for as soon as England is directly united with the European continent, practical advantages of incalculable value will result. The resources of each country, and mutual exchange of produce, will be developed to a degree of which it is impossible to form anything like a correct idea. Mr. Chalmers went very carefully into the subject of the probable revenue to be derived from the establishment of a Channel railway. Writing in 1867, and referring to his estimate, he says: "I published these figures in a brief prospectus of this project five years ago; and I see no reason to alter them now, unless to increase them. After I adopted them I became acquainted with the work of M. de Gamond, and on comparing figures, found a wonderful coincidence in the items of freight and passengers,

	£	s.	d.
His being . .	1,041,666	13	4
And mine . .	1,049,375	0	0

He seems to have overlooked the mails* as a source of revenue; his figures were compiled in 1856, and based on the actual business then done between England and the Continent; and as he could not have foreseen the impetus that has since been given to traffic between England and France by the recent Treaty of Commerce, and by the change in the passport system, his figures are more sanguine than mine, compiled in 1861, after these important changes had actually taken place."

Mr. Chalmers estimates the probable revenue as follows:—

	£	s.	d.
2500 tons freight daily, or 912,500 per aunum at 12s. 6d.	570,312	10	0
1500 passengers daily each way, or 1,095,000 per annum at 8s. 9d. .	479,062	10	0
Mails, express freight, coin and bullion, extra baggage, &c. (say) . . .	250,645	0	0
Total annual revenue . .	1,300,000	0	0

These figures, he observes, may appear too high to those who have overlooked the affinity between improved means of transit and the increase of traffic resulting therefrom. In the infancy of railway enterprise, the anticipated traffic on a given line was based upon the business done by the stage-coaches and waggons of the day. It needs not that we compare the anticipated with the actual railway traffic, but we should bear in mind that our experience in that case did not prevent our falling into a similar error in the case of ocean steamers, though fleet after fleet of these vessels have taken their places on the ocean, each creating for itself a trade where none such existed before. Between 1820 and 1830, in the good old times of sailing packets, the number of travellers between England and the Continent did not exceed 80,000 per annum. The establishing of a regular steam-boat service raised the number in twelve years to 350,000; and since the introduction of railways, it has risen to upwards of a million. This great increase is not to be attributed to the increase of the populations, but mainly to these improvements; and the effects that would result from the completion of any work connecting England with the Continent would be even greater than were produced by those two important revolutions in locomotion, which respectively raised the figures from 80,000 to 350,000, and from the latter to upwards of a million.

* Mr. Chalmers and M. de Gamond have both omitted the revenue which would now be derivable from the electric telegraph companies.

In confirmation of these views we find Captain Tyler stating, in a recent report upon the improvement of the means of communication between England and France, that, omitting from consideration the ports of Hamburg, Rotterdam, Antwerp, and Ostend, the passenger traffic between England and France for the year 1868 amounted to 309,479 altogether; 141,633 passengers having travelled by Calais, 109,006 by Boulogne, 41,371 by Dieppe, and 17,469 by Havre. A large proportion of these, namely, 46,411, crossed the Channel in the month of August, as against 12,946 in January, and 13,514 in February. In the year 1867, of the Paris Exhibition, the numbers were 454,350 altogether; 199,837 having travelled by Calais, 146,226 by Boulogne, 86,914 by Dieppe, and 21,373 by Havre. Of these, 84,684 crossed in August, against 13,163 in January, and 13,721 in February. Captain Tyler observes that in addition to the ordinary annual increase, which is considerable, there would naturally be a very large augmentation in these numbers if better arrangements were made for crossing the Channel.

Conclusion.

We have definitively before us three propositions for effecting the desired communication by means of an excavated tunnel, three for laying a tube on the bed of the Channel, three for improved steam-vessels, and one for a bridge, besides several other propositions of minor importance and questionable practicability. Of all these projects undoubtedly those which have of late made the greatest progress are the bridge scheme of M. Boutet, and the tunnel scheme of Messrs. Hawkshaw, Brunlees, and Low. The documents relating to this latter scheme are now before the Board of Trade. The results of the deliberations of a French commission appointed by the Emperor to inquire into the scheme are, on the whole, favourable as regards the geological and engineering features of the project. In this scheme it is proposed to commence by driving preliminary driftways through the grey chalk, at a great depth below the bed of the Channel, between a point near Dover and another point near Calais. It is conceived that this material would be easily cut through, and would not be likely to present insuperable difficulties from the influx of water. Mr. Remington, as we have already seen, selects the line from Dungeness to Cape Grisnez, in order to avoid the chalk and the fissures which he fears to encounter in it, and to work in the Wealden formation, which would, he believes, afford a greater chance of success.

Turning to M. Boutet's bridge scheme we find that an association has been formed for making experiments, two small bridges have been built in France, and arrangements are made,

near St. Malo, for a third, whilst a fourth is to be constructed over the Seine. The great features of the bridge are that it would be less costly than a tunnel; will occupy less time in construction; will give no trouble in ventilation; and will avoid the danger of sudden inundations. As to its merits in an engineering point of view, we may hope shortly to be satisfied by practical demonstration.

Looking broadly at the schemes which present the most reasonable features, and irrespective of their engineering merits in detail it appears to the author that, of the tunnel schemes, that of Mr. Remington for driving through the Wealden formation would be attended with less danger than that of Mr. Hawkshaw, which it is proposed to carry through the chalk. Of the methods of connecting the two shores by tubes along the bed of the Channel, that of Mr. Bateman certainly appears the most practicable. If these tubes could be constructed in a dry dock and drawn gradually over upon Mr. Colburn's method, it would be a very summary method of settling the question, as Mr. Colburn assures the author he could effect the connexion in three months, although, he admits, at a great cost. But, both in subterraneous and subaqueous works, there is an admitted possible risk. In the former, there is the contingency of flooding from the nature of the soil, whilst, in the latter, some of the operations would be dependent on comparatively delicate arrangements. The bridge scheme has also its perils of storms and tempests, but there appears to be a possibility of guarding against the consequences of these more readily than against the insidious advances of a great head of water. The bridge scheme, too, as has already been observed, has had its substructure approved by one independent engineer, and its superstructure by another. As far, then, as we have at present advanced, the bridge scheme appears to present the most reasonable chance of success. But either a tunnel, a tube, or a bridge would be the work of perhaps eight or ten years, for the author does not think the various projectors have allowed sufficient time for the contingencies that would arise in the course of carrying out works of such an unparalleled magnitude. We must, therefore, turn to some plan by which the existing requirements of the travelling public can be promptly and inexpensively met.

Captain Tyler, R.E., has examined the English and French coasts and investigated the various projects, and has reported to the Board of Trade thereon. Referring to Mr. Fowler's plan for improved steam-vessels and harbour accommodation, Captain Tyler observes that the project would require some modifications in detail, and that it is a question whether it would be worth while to ferry the railway carriages as well as the passengers

across the Channel. But the main features, of an improved harbour at Dover and a new harbour south of Cape Grisnez, are sound, if means can be found for meeting so great an expense.

With regard to Mr. Grantham's proposition to utilise the existing harbours by vessels of light draft, Captain Tyler states that it is asserted by some of the officers engaged in the performance of these services that vessels of the class now employed are, upon the whole, the safest that could be devised for the particular duties required of them. It is argued that the sea-passage, in which greater length and size might lead to increased comfort, is comparatively short, while the entrance of the French harbours, by day and night, in certain states of the weather, which is already the more difficult and dangerous part of the service, would be attended with still greater disadvantages. The existing vessels are fitted to encounter any weather with which they can meet in the Channel, and are handy for entering the harbours, while longer vessels would be exposed to increased risk at the moment of entering the harbours. The bow of a long vessel getting under the shelter of one pier, and a heavy sea striking her on the quarter, she might be driven against the other pier. There is, no doubt, much truth in this argument. Such a risk must increase in proportion to the length of a vessel, to the narrowness of the entrance to a harbour, to the exposed position of its entrance as regards winds and currents, and to any necessity which may exist for reducing the speed before entering it. Having regard to the existing state of the French harbours, the employment of longer vessels would, for this reason, be attended in bad weather with greater danger. But the argument would no longer apply if the service were performed from a convenient and well constructed pier on the one side, to a similar pier on the other side of the Channel. The longer and larger vessels, having increased proportional power, with disconnected engines, so as to be able to use either paddle at pleasure, could be under no disadvantage in approaching the lee side of a pier. The above argument, therefore, tells in favour of the construction of an extended pier at Boulogne, as proposed some time since by Mr. Brunlees to be used on the French, in combination with the pier at Dover on the English coast, for an improved Channel service.

The matter, then, in general terms stands thus: The steam packet service between England and France is greatly in need of improvement. This service is important in its character, and the existing steamers, restricted as to their dimensions for want of better pier and harbour accommodation, are not proportionate to the importance of the service. Larger vessels, with less movement in rough weather, more shelter, and better accom-

modation generally, would do much to mitigate the discomforts of the sea passage; and even contemplating the successful issue of a tunnel or bridge project, these improvements are much required, and should be effected in the mean time. But larger vessels cannot be employed for a fixed service until better provision is made for embarking and disembarking passengers, especially on the French coast. The pier at Dover is not only ready, but has frequently been used for military transport-vessels of the largest size, though certain improvements are required in the jetties for greater convenience in embarkation and disembarkation. The difficulties in the way of fitting the harbour of Calais for the reception at all times of larger vessels are great, but by a judicious extension of the west pier at Boulogne, similar accommodation might be provided on the French side. Captain Tyler estimates that at a cost of about 100,000*l.* at Dover, and 500,000*l.* at Boulogne, the desired object might, apparently, be attained in the most economical and most expeditious manner. By the adoption of steamers capable of moving with equal facility in either direction, the difficulties incidental to turning round in small harbours may be avoided; and the existing harbours at Dover and Boulogne might, with certain modifications, be made available, to some extent, for improved vessels.

Captain Tyler suggests that the question might thus be temporarily solved if the two governments were not prepared to carry out a larger scheme. If they are prepared to face a greater expense, there is the more extensive project of Mr. Fowler, for constructing, at a cost of two millions of money, special harbours for special steamers. There can be no doubt that this project presents—whether the railway carriages be ferried over or not—a more comprehensive mode of dealing with the subject. The relative distances may be stated to be: Between Dover and Audresselles, twenty-four, Dover and Calais, twenty-six, and Dover and Boulogne, thirty English miles; while the passage to Boulogne, though longer, is admitted to be better than that to Calais. Either by the construction of new harbours at Dover and Audresselles, at a cost (as estimated by Mr. Fowler) of 2,000,000*l.*, inclusive of steamers, or by the improvements above suggested at Dover and Boulogne, at a cost of 600,000*l.*, exclusive of new and improved steamers, the immediate object should be to provide an improved fixed service, irrespective of wind and tide, between London and Paris in eight hours. It would appear to be desirable now to refer the whole matter, through the Foreign Office, for the consideration of the French Government, with a view, perhaps, to the appointment of an International Commission, in which that government would, no doubt, readily acquiesce, and

without which the important interests involved could not be authoritatively dealt with, nor the general question satisfactorily decided.

Whatever be the plan ultimately decided upon for connecting England and France in a direct manner, such a plan must have the best wishes of those here present this evening, as well as those of the whole civilised world. It should not be so much the honour of adopting this, that, or the other scheme that should influence us, as the reflection that the accomplishment of the object will be attended by advantages to the nations of the earth. In former times, when Europe was regarded mainly as a theatre of war, it was, perhaps, no disadvantage for this country to be separated from it. But, since the introduction of steam has so completely changed the character of marine locomotion, any advantage formerly arising in this respect from our insular position has been materially diminished, and, in the present day, it is continental commerce from which we are separated and not continental wars. Such a direct bond of union, then, as is proposed would establish on the firmest basis the spirit of amity now subsisting between England and France.

November 1st, 1869.

F. W. BRYANT, PRESIDENT, IN THE CHAIR.

ENGLISH AND CONTINENTAL INTERCOMMUNICATION.

DISCUSSION.

Mr. NURSEY said that before the discussion was opened he wished to offer a few remarks by way of explanation. His attention had been drawn to a passage in his paper, referring to Mr. Colburn's method of laying tubes to form a tunnel for communication across the bed of the Channel. In his concluding remarks he said, with regard to Mr. Bateman's scheme :—"If these tubes could be constructed in a dry dock and drawn gradually over upon Mr. Colburn's method, it would be a very summary method of settling the question, as Mr. Colburn assured the author he could effect the connexion in three months, although, he admitted, at a great cost." It had been stated to him (Mr. Nursey) that it might be inferred from this passage that Mr. Colburn could commence the work and complete it out of hand, tunnel, and everything, in that time. This statement he thought it right to say, presupposed the assemblage on the ground of all the materials and all the parts, and the construction of a length of dock sufficient to contain perhaps a mile of these parts when put together, that the operations might be continuous, and that there might be no delay whatever in any parts of the work. Therefore, it was not the summary process it might appear to be to some casual readers without the slight explanation he had made.

Mr. W. WILSON said the subject of international communication was one to which he had devoted a very large amount of attention. He had studied it for some years, and had investigated every scheme that had been brought forward. His

opinion was that they were all perfectly practicable and feasible, provided the means for carrying them out were forthcoming. He did not, however, believe that either a tunnel or a bridge could be made commercially successful, as there was not sufficient traffic existing, or likely to be created, that would ever pay a return upon the outlay. In consequence of that he had come to the conclusion that there was but one means of meeting the requirement, and that was a scheme with which he was associated for providing large and powerful steam ferry-boats. There was no doubt that scheme could be carried out in such a manner, and at such a cost as to be a commercial success. The steam ferry-boats were perfectly practicable and comparatively economical, but among the tunnels or tubes were objections arising from international questions. The expenditure of so large a sum as ten or twelve millions, in a matter which might at any moment be destroyed or rendered perfectly useless, in case of a breach of the peace, was a question for very grave consideration, and was an objection applicable to either a bridge or other fixed structure.

Mr. Olrick took objection to Chalmers' tube of boiler plate lined with brickwork, as, in the event of an engine leaving the rails, or a collision occurring, the iron shell might be damaged, and the tunnel filled with water. There was also the liability to accident from external corrosion. It was a great question whether such a costly undertaking would ever pay. A tunnel was better than a tube, but Hawkshaw's scheme was open to objection, on account of the fissures to be met with in the chalk formation. The only scheme he considered feasible was that which proposed improved harbourage and larger steam-boats. Boutet's bridge was deserving of complete condemnation, for it was an impossibility in every respect.

Sir Thomas Parkyn said with regard to the feasibility of the bridge scheme he might mention that he had consulted Mr. Ordish, who told him that he not only considered the piers quite practicable, as regarded their manufacture, but also that he considered the floating them, and the placing them in position in the Channel, could be effected. He had taken a great interest in the bridge scheme, and believed in the ultimate success of M. Boutet.

Mr. W. Austin stated that he brought his scheme for a tunnel before the Institution of Civil Engineers thirteen years ago, and was then told that he was premature. The public, however, were now demanding better accommodation, and engineers were studying the best means of giving it. Ferries he considered were of no avail, the public were tired of the break of route and of the rough passage which the best boats would not

alter. The proper scheme was a tunnel; the times demanded it, and nothing but an unbroken route would satisfy the public. He would suggest the formation of an International Committee to consider and decide upon the question. There could be no fear of his proposed tunnel not proving a commercial success.

Mr. CARGILL said that the question of intercommunication involved the consideration of some of the largest schemes that had ever been brought under the notice of the engineers of any country. It was not so much on account of the distance, as the nature of that distance, which constituted the great difficulty to be surmounted. There were four or five different methods proposed to cross the Channel. The only scheme that was assumed to be impracticable was the bridge scheme. In looking at a scheme embracing many novelties like the bridge, it would be well to consider what had already been done. It was acknowledged that the substructure—the foundation—was perfectly feasible. He did not adduce the example of the Britannia Bridge, because there the work was not under water; they had merely to prepare for the masonry; but he thought that the Saltash Bridge over the River Tamar was a case in point which he might instance. The tide was exceeding rapid at the mouth of the river, and the pier was got in under great difficulties. Passing from the substructure to the superstructure, he regretted he had not been able to obtain detail particulars of the bridge. But, although the span was great, it was to be remembered that there was a bridge in course of erection in America having a span of more than a third of a mile. The Boutet bridge was stated to have spans of two-thirds of a mile, which was simply an extension, which, in the progress of engineering science, might reasonably be looked for. Statements had been prepared as to what the traffic was likely to be, but there was at present no data for calculating the traffic, and it could not be known what it was until the communication was made.

Mr. ALFRED WILLIAMS said that any scheme of intercommunication must be such as would give the greatest amount of comfort and accommodation to the public. His own opinion was that the quickest, safest, and the best method of crossing the Channel was by means of ferry-boats, as proposed. Whether they carried the train or not, was a matter of little importance. He believed that appropriate boats could be built, and that a ferry was the scheme that would be carried out, and for the following reasons:—First, that it could be easily done; secondly, that it was very much cheaper than any other scheme that had yet been brought out; and thirdly, that it would be the most convenient and comfortable to travellers.

Captain ROBERTS observed that the ferry scheme was open

to the objection that, the longer and deeper the vessel, the more difficult she became to pilot into harbour in rough weather, as there was the chance of her being struck by a sea, or swung round by a current. With regard to the bridge scheme, he was acquainted with the Boutet system, which had proved so far successful in a small example, and might be expected to do so on an extended scale.

Mr. WILSON said that with regard to the practicability of the vessel entering the harbour, in his plan the opening between the two piers was 400 ft., so that there was no difficulty whatever in entering, even in rough weather. The boats when loaded would draw about twelve feet of water, and that draught would give them that steadiness which was so desirable. The goods traffic had occupied the fullest possible attention, and arrangements had been made for carrying goods and passengers simultaneously. The goods trains were carried below and the passenger trains above, the boats being capable of conveying about fourteen ordinary railway trucks, in addition to twelve passenger carriages placed on the main deck. The railway trucks with the goods not being so urgent a matter, and not requiring the same expedition as the passengers, could be put on board during the interval that the boat was in the station waiting the arrival of the passenger train. There were mechanical arrangements proposed by which this was very easily managed.

Mr. NURSEY said that he was glad to have heard from Mr. Wilson that he considered no scheme impracticable in an engineering sense. That was one of the conclusions of which he (Mr. Nursey) had arrived while preparing his paper. Mr. Wilson had supported the ferry scheme, and very properly so, as it was a reasonable and ready method of overcoming the present difficulty. Mr. Olrick mentioned that Mr. Chalmers' proposed tube was liable to damage and destruction from within and without. He would recal to his (Mr. Olrick's) recollection the fact that the tunnel proposed by the late Mr. Chalmers was covered in, in fact, it ran through an embankment, and was well lined with brickwork. Mr. Olrick spoke of the deterioration that might take place in the iron. That might or might not be, but in his paper he (Mr. Nursey) had met that point by referring to a statement which was made by M. Maury in his physical geography, which he would repeat:—"Count Marsigli divides sea-water into surface and deep sea-water, because when making salt from surface water, not more than six inches below the upper strata this salt will give a red colour to blue paper, whereas, the salt from deep sea-water will not alter the colour at all. The blue paper can only change its colour by the action of an acid. The reason why this acid (iodine?) is found in surface, and not in deep sea-water, is that

it is derived from the air." Hence, argued Mr. Chalmers, the bottoms of iron ships when the interior skin of the iron was kept clean and well painted, appeared to suffer little or nothing from the action of the water on the exterior, and he went on to say that, by parity of reasoning, the iron of his tubing would not suffer even if the salt water should percolate through the stratification and get to it. Much discussion had taken place on Boutet's system of bridge. There had been some very strong opinions pronounced against it, and it was a matter of regret that he (Mr. Nursey) was not able to place before the meeting a full and correct engineering description of the principle which would have been then open to fair engineering criticism. As he understood it, the system had only very recently been perfected, and was in course of being patented, and the specification, he thought, was not quite complete, or at any rate not published. Had publication taken place they would have been in the possession of full particulars, and able to discuss the question upon more definite grounds. He would confirm what had been mentioned respecting Mr. Ordish's opinions as to the piers of the proposed bridge, because Mr. Ordish had expressed the same opinion to him, Mr. Nursey. Mr. Ordish stated that the scheme of a bridge, not necessarily Boutet's bridge, was feasible and practicable. As Mr. Cargill had observed, it was not because they had attained to a certain point or limit in engineering science that they were to stop at that limit. Mr. Ordish had proposed a suspension bridge on his rigid suspension principle across the Thames from the Tower. Mr. W. H. Barlow had reported that the bridge, as designed, was perfectly safe, and he (Mr. Nursey) believed Mr. Barlow had also embodied in his report a statement to the effect that Mr. Ordish's principle was safe and practicable up to about half a mile span, he having carefully taken out the strains of such a structure. If that principle was practicable he did not see why any one should pronounce, as impracticable, a system which had been satisfactorily illustrated in model, and the full details of which were not yet made public. Turning to the whole question of intercommunication between England and France, it was to be summed up, he thought, as followed :—The public were in very great want of better means of getting across the Channel. To supply that want by any of the tunnel or bridge schemes now before them, would occupy many years and involve an enormous outlay, beyond anything he had yet seen committed to paper. He did not go into the question as to whether it would pay or not, but his own conviction was that it would pay very well ultimately, though it might be a work of time before it would succeed as a commercial question. There were two underground

tunnels proposed, and of these, he thought that that through the Wealden formation would be the safest. As regarded the tubular schemes, he was not much in favour of them. Coming to the question of communication by means of improved packets, they had two alternative schemes, the principal one being that of Mr. Fowler and Mr. Wilson, which would cost two millions, and really a very excellent plan it seemed. The other scheme, or rather proposition, for it amounted to nothing more, was that given in a report by Captain Tyler, in which he stated that by improving the west pier at Boulogne at a cost of 500,000*l.*, and by improving the Dover pier at a cost of 100,000*l.*, and starting between those two points a better class of boats, they might obtain all that they required for present purposes. It certainly seemed to him (Mr. Nursey) that while they were waiting for a tunnel, a tube, or a bridge, the most sensible course would be to adopt one or the other of these boat schemes. He thought those were schemes to which they should give special attention pending the perfecting of the other ideas. Tunnels, tubes, and bridges were practically schemes of the far future. For the present engineers must consider how best they could meet the necessities and urgent requirements of the case, and he thought they could be well met, temporarily though it might be, by the improvement of the harbour accommodation on either side of the Channel, and the adoption of more commodious steam-vessels.

The PRESIDENT said that with regard to the bridge, he thought that something more should be known of its details before an opinion could be formed of its merits. As to the tunnel schemes, the great objection seemed to be, according to the discussion, the great outlay which a tunnel required, and which would prevent it from becoming a commercial success. The steam ferry across the Channel appeared to be generally approved of. The outlay would be less than in any of the other schemes, and he had no doubt that it was the most practicable in all respects.

November 15*th*, 1869.

F. W. BRYANT, PRESIDENT, IN THE CHAIR.

THE NEED OF FURTHER EXPERIMENTS ON THE STRENGTH OF MATERIALS.

BY CHARLES J. LIGHT.

ALL sound engineering practice must necessarily be based upon experimental research into the strength of the materials employed, whether these experiments are purely scientific, and conducted with the accuracy and precision necessary to qualify them for supplying data on which to base formulæ for general use, or of a rough and ready character hardly to be recognised as experiments. For even the most ordinary proportions of parts must originally have been based upon trials—very often upon failures. It is with the former class only that the author now proposes to deal, and to point out how far, in some very important branches, it has hitherto failed in answering its proposed end. Two essentials of useful experiment are, that all action whatever that could interfere with the sole development of the strains to be investigated should be carefully eliminated, and that the apparatus used should possess the means of measuring the results with the utmost possible accuracy and delicacy. These points are essential, because where they are neglected, it becomes difficult to say what are even the limits of error, much less what corrections have to be applied to the results obtained. If this be thought too much of a truism, it will be sufficient to refer to the practice, once almost universal, of applying tensile strains by hydraulic pressure, and measuring the force by means of a gauge connected directly with the press itself, when the tion of the cup leathers introduced an element of uncer- ty which entirely vitiated the accuracy of the results. When object of the experiments is to establish formulæ, it is clearly essary that, besides the above-named conditions of the appa- , the samples operated upon should be prepared with tmost care and precision: but the author would suggest t the same time samples should be prepared from iden-

tically the same materials, but under as nearly as possible the ordinary conditions of manufacture, so that a comparison of the two classes might afford data for determining what proportion of the theoretical strength might reasonably be calculated on in practice.

Of late years great numbers of experiments have no doubt been made, especially upon the resistance of iron to tensile and transverse strains, but they have been generally for private purposes, and have not been allowed to become known. A noteworthy exception, however, is the valuable series of experiments in wrought iron and steel carried out by Mr. Kirkaldy for Messrs. Napier, and so fully published with their permission. Upon the questions of tensile and transverse strains, therefore, it may be said that our information is fairly complete, but on the highly important subject of compressive strains it is admitted on all hands that reliable data are sadly needed. In a recent communication to the Institution of Civil Engineers, a French authority, M. Gaudard, remarks that "the case of a pillar pressed upon its bases is very imperfectly understood. We are almost reduced to the application of formulæ purely empirical, which aim rather at conforming to observed facts than at explaining them." Whether the formulæ we have to rely upon deserve even the modified description of "conforming to observed facts" may well be doubted after a very slight examination and comparison.

It is surely a matter of great surprise that the best series of experiments into the strength of columns and struts dates as far back as the year 1840, when Mr. Eaton Hodgkinson tested a considerable number of small columns of various diameters and proportions with the express object of determining practical formulæ. Since then nothing further has been done in this direction, and the results at which he arrived are still the admitted basis of calculation for columns and struts. It might, therefore, have been expected that these results were so clear and satisfactory as to render further investigation unnecessary, which, however, is very far from being the case. For example, the well-known formula for long hollow columns with both ends fixed, that is, according to Hodgkinson, columns whose length exceeds thirty diameters, is—

$$W = 44{\cdot}34 \frac{D^{3{\cdot}55} - d^{3{\cdot}55}}{L^{1{\cdot}7}}$$

Where W = breaking weight in tons.
D = external diameter in inches.
d = internal diameter in inches.
L = length in feet.

On seeing so peculiar an index or exponent as 3·55 in a professedly empirical formula, it is natural for the student to suppose that the experiments rendered results at least very closely approximating to the index finally adopted. What, therefore, must be his surprise to find that in fact the indices obtained from the various experiments ranged from 3·922 to 3·412? (see the foot-note to page 333 of Mr. Hodgkinson's work). That is, if the external diameter be 12 in., the factor to be employed might range from $12^{3\cdot922}=17{,}082$ according to one experiment, to $12^{3\cdot412}=4810$ according to another. The same remark applies to the factor $L^{1\cdot7}$, as it appears that the index varied from 1·914 to 1·424.

In the sixth edition (1867) of Barlow on the Strength of Materials, edited by Mr. Humber, a short article on cast-iron columns has been inserted, which is an almost verbatim extract from Hodgkinson's work above referred to, and in which his formulæ are adopted as the best yet known, with a modification suggested by Mr. Hodgkinson, viz., taking the index of the diameter as 3·6 both for columns with fixed and with rounded ends. It is somewhat anomalous that, while adopting this modification, the effect of which is very considerable even in columns of moderate diameter, what may be almost called the affectation of extreme accuracy is retained in the constants of 44·3 for hollow and 44·16 for solid columns, where it is obvious that the decimal part exercises an influence upon the result perfectly insignificant as compared with that of the slightest change in the indices.

Another serious cause of uncertainty arises from the admittedly different kind of resistance offered by very long and very short columns, the one being principally a resistance to crushing and the other to flexure. Mr. Hodgkinson came to the conclusion that his formula for long columns could be adopted to short columns, at least for certain classes of iron, by means of the following expression:—

$$\text{Breaking weight of short column} = w.$$

$$= \frac{W \times C}{W + \frac{3}{4} C}$$

where C is the crushing force in lbs. belonging to the material employed ectional area of the column. As, however, C will in ry cases be very large in proportion to W, the expre ally resolves itself into $w = \frac{3}{4}$ W.

N ous that this is a most unsatisfactory solution, for upposed that there is any point at which a su so considerable an extent takes place. Or, to 1 to the test of an example, let it be supposed

that the column whose breaking strength is sought is 30 ft. long, 12 in. external, and 12 in. internal diameter, is it to be classed as a long or a short column? In the former case, its ultimate strength would be, according to Hodgkinson's formula, 442 tons, in the latter 589. Nor, if it be admitted that there are two such distinct classes as long and short columns, is there any certainty as to the proportion at which the change occurs, other authorities placing it as low as five diameters in the case of cast iron.

Leaving for a time the consideration of Hodgkinson's formula, the next that claims attention is that given by Professor Rankine:—

$$W = S \frac{f}{1 + \frac{l^2}{cr^2}}$$

Where S = sectional area of a column, l = its length in inches, r the least radius of gyration of its cross section, f and c two coefficients depending upon the material, and appearing to have relation to its powers of resistance to compression and tension. It will be seen that no comparison can be made between this formula and Hodgkinson's, but it is evident that Rankine's bears a more purely theoretical character, and that it involves the fourth power of D, in which respect it agrees with that originally laid down by Euler, which Hodgkinson thought could only be reconciled with practical results by modifying the index as before noticed.

Besides these formulæ, there is also one quoted by Molesworth, though from what authority it was derived the author has been unable to ascertain. It is given on the nineteenth page of his Pocket-book, and called an "Approximate Rule for the Strength of Iron Struts," without any limitation as to the proportions between their length and diameter. For the case of hollow columns of cast iron this rule gives—

$$W = S \times 36 \div 1 + \frac{l^2}{400\,D^2}$$

$$\text{or} = S \frac{36}{1 + \frac{l^2}{400\,D^2}}$$

Rankine's formula adapted to a similar case would be

$$W = S \frac{80{,}000}{1 + \frac{8\,l^2}{3200\,D^2}} = S \frac{80{,}000}{1 + \frac{l^2}{400\,D^2}}$$

so that the forms are identical, while the difference of the coefficient is, in this case, but trifling, as Rankine's formula gives the value of W in lbs., and 80,000 lb. = 35·715 tons. When, however, the material of the column is wrought iron, the similarity of form continues, but the coefficients differ so greatly as to cause great discrepancy between the results, a discrepancy varying with the relations between the length and diameter. The same remarks apply to the formulæ given for columns of a hollow square and a cruciform section. To assimilate the formulæ for wrought iron to those of Rankine, they must be altered thus:—

For cruciform section, $16 \div 1 + \frac{l^2}{1500\,D^2}$

For hollow square section, $16 \div 1 + \frac{l^2}{3000\,D^2}$

For hollow cylinder section, $16 \div 1 + \frac{l^2}{4500\,D^2}$

In order to show how great is the variance between the different formulæ, and therefore how uncertain any calculations of the strength of columns must be, it will now be desirable to give the results in a few practical instances. First, let the case be that of a cast-iron column 30 ft. long, 12 in. external, and 10 in. internal diameter, with the ends flat. Hodgkinson's formula will give its breaking weight, if it be considered as a long column—

With index 3·55	442	tons.
With index 3·6 (said to be sufficiently near for practice	505	„
If taken as *short* column	589	„
Or the same with index 3·6	631	„
According to Rankine's formula, taken as long column	380	„
According to the formula on page 19 of Molesworth .	383	„

Which of these results, the extremes being as 5 to 3 very nearly, is to be taken as the true breaking weight of the column?

Again, take the case of a column, also of cast iron, 30 ft. long, 24 in. external and 20 in. internal diameter, with the ends rounded, or, what is the same in effect, jointed. Hodgkinson's formula gives, with what he considers the true index for D, viz., 3·76—

Breaking weight	3080 tons.
With the index 3·6 ("quite near enough for practice," let it be remembered!)	1797 „
Or, if it be taken as a *short* column, which at the proportion of 15 diameters to the length, we have a right to do	4107 „
Taken as a long column, by Rankine's formula . .	1519 „
By formula, page 19 Molesworth	955 „

In this case, the extreme values obtained for W are to one another as 43 to 10, very nearly. How is it possible to arrive at any idea of the real strength of such column, which is yet one of very probable dimensions? If, however, wrought iron be the material employed, the discrepancies become still more startling. Assuming the case of a similar column to the one first instanced, only in wrought iron—

Hodgkinson would give	771 tons.
Rankine	463 „
Molesworth	170 „

The first result is based on the statement that the relative strengths in long columns of wrought and cast iron are as 1745 to 1000—an hypothesis which must be entirely erroneous, for the ultimate crushing strength due to the area of this column, without any consideration of its liability to flexure, is only 586 tons (calculating at 38,000 lbs. on the square inch).

The uncertain results of Mr. Hodgkinson's experiments must, there is no doubt, be chiefly attributed to the small size of the columns operated upon, and not to any want of skill or care on the part of the operator. The largest solid column tested was only 5 ft. long and 2 in. diameter, and in hollow columns the greatest length did not exceed 7 ft. 6 in. with a diameter of 3½ in. The more powerful means of testing now at the command of engineers would allow of experiments being made upon a much larger scale, and, it is reasonable to anticipate, with far more uniform results.

Before leaving this part of the subject it will be well to notice a remark of Mr. Hodgkinson ("Phil. Trans." 1840) respecting cases of imperfect casting in hollow columns, where the metal on one side was considerably thinner than on the other. He says: "It is gratifying to find that a matter which would seem to destroy all confidence in a pillar does not produce a great reduction in the strength." This apparent anomaly he explains by supposing that by some natural law the thinner side always takes up the compressive strain, and the thicker the tensile,

and the instances that actually occurred among his experiments certainly confirmed this supposition. This will be an important point for future observation.

In reference to wooden pillars or columns, it will be sufficient to remark that the best established formulæ give results which differ from each other in a similar manner to those for iron, though not to quite the same degree.

Enough has probably now been said to show how impossible it is, with our present information, to determine within anything but the roughest approximation what load any given column ought to bear before breaking. It is true that even if strict accuracy were attainable in this respect, there would yet remain the variations of material and workmanship to be allowed for, besides the important question of the factor of safety that should be adopted. But the author would urge that we ought not to be content until a theoretical basis has been established as certain and rational as that upon which calculations of transverse strains are now based.

Another class of experiments, much more readily to be made, and of a much simpler character, would yet be of great practical utility. Their object would be to determine the resistance to transverse and compressive strains of wrought-iron L and T bars of various sections, and to deduce therefrom practical rules. It is very probable that ample materials already exist in private hands for compiling such information, and, if not, the necessary experiments could be easily and cheaply made. This is not a case requiring extreme accuracy, especially as it would be the actual full-sized bars that would be operated upon, but every engineer ought to have the means of determining at a glance what load any given bar will properly bear. Such tables would be the means of saving many a ton of metal in roofs and flooring, &c., and would afford the designer an assurance of the efficiency of his structure, which he cannot at present be said to have.

The next series of experiments to the need of which the author desires to call the attention of the Society, would require very great care and skill, both in arranging and in carrying out. They refer to the true proportioning of the eyes of links. It is now many years since attention was first directed to the necessity for providing, not only a sufficient shearing area in the bolts or pins securing links, tie-rods, &c., but also a sufficient bearing area of the link or tie itself upon the bolt, a point which appears to have been overlooked, at least in civil engineering, until the rapidly extending use of iron roofs, and the desire to reduce every part to the lightest possible form, brought it into prominence. And even now there is great difference of opinion as

to the extent of such bearing area, some authorities contending that the crushing strain so induced ought not to be allowed to exceed (for wrought iron) five tons to an inch, while others would extend the limit to seven or eight tons. The correct proportions for the eyes of links, whose thickness is small in proportion to their breadth, as in those of bridges, still present a vexed question; and it is not easy to determine how far they are affected by the unavoidable irregularities of manufacture.

Experiments appear to show that the proportions at present adopted in the best practice, though greatly modified from those at first used, are even yet insufficient to secure the fracture of the link through the body in preference to the eye. The author submits that it would be highly desirable to arrange and carry out a series of experiments on links of graduated cross sections, from the thin link, as employed in the lower members of bridge girders, to one of a square section, with a view to determine the proper proportions for the eyes under these varying conditions. Is is, however, quite probable that a large amount of information already exists suitable for such a determination if it could only be brought together under such conditions as to permit of a fair comparison of results.

Upon the strength of timber as a material of construction there have been made so great a number of experiments that it may be doubted whether fresh ones would add much to our knowledge of the subject. It may be remarked, however, that the greater part of these, also, are open to the objection before referred to of having been made upon exceedingly small pieces. It is also true that with timber so very much depends upon the age, the growth, and the place of growth of the tree, that it can never be expected that calculations for this material can be brought within the same limits of accuracy which may be fairly looked for in the case of iron.

Further experiments are probably desirable on the strength of combinations of timber and iron, such as trussed beams, keeping in view the consideration that the timber must necessarily yield to a considerable extent before any appreciable strain is brought upon the iron tie. This objection is of much greater force in the case of the flitch girder, in which the timber ought only to be looked upon as giving lateral stiffness to the iron plate.

In reference to the important question of the strengths of stone, brick, cement, and artificial stone, there is still room for a well-arranged series of experiments, especially if made upon much larger samples than those which have hitherto been operated upon. Another interesting subject for fuller experiment is that of glass, as laid in sashes both of wood and iron. In the latter case observations should be made under varying tem-

peratures. Several other branches of the inquiry will doubtless be suggested in the course of discussion, and the object of this paper has been, not so much to call the attention of engineers to a want which the author is well aware is already known and admitted, as to elicit from all who take an interest in the subject such information as they can contribute to the common fund, and such suggestions as may lead to a practicable remedy for a state of things which cannot be considered creditable to the present position of engineering science.

If the foregoing fairly represents the present state of our information upon this important subject, it remains to consider what means can be suggested for satisfactorily supplying the deficiency, and obtaining data upon which reliable formulæ might be based. The best course would probably be for an adequate series of experiments to be carried out covering the whole field of inquiry on any one branch of the subject—say that of columns and struts—to be conducted under the direct superintendence of two or more engineers of such standing as would give weight and authority to the results at which they might arrive. But this course would necessarily involve a very heavy expense, such as no individuals, and probably no body of engineers even, would be inclined to bear. The author would therefore suggest that the same desirable result may be attained by organising a system of co-operation, whereby the expense may be so spread as not to be felt a burden. To this end three things appear to be essentially necessary. First, that all the experiments should be conducted according to a prearranged form, and by means of one and the same testing machine. Secondly, that with each experiment on strains of compression a piece of standard size from the same melting as the castings to be experimented on should be subjected to the test of crushing, so as to furnish a datum of comparison with other cases, and similarly of course with other materials and other classes of tests. Thirdly, that there should be a central body or committee to which all information should be sent, and by which it should be analysed and sifted, all that might be thoroughly reliable classified and tabulated, and finally its results carefully formulated.

The author would suggest that the Society of Engineers might well take the initiative, and appoint, through its Council, a committee to act as such a centre; that any member of the Society holding the position of engineer to works of sufficient magnitude should use his influence to have a small sum set aside, either by the principals or the contractors, for the purpose of experiment, the special object of which should be arranged with the central committee; and that the co-operation of engineers not connected with the Society should be invited for the same object. The objections anticipated are, that where testing

is resorted to in connexion with public or private works of construction, it is generally for the private purposes of the parties concerned, and that there might probably be great disinclination to allow the materials used to be experimented on if the results were to be made public. Again, it may be feared that the engineers most qualified to form the proposed central committee would not be able, assuming that they would be willing, to devote the necessary time to the work of arranging and formulating the information sent to them.

The author would meet these difficulties by the consideration that if the first-named condition, insuring uniformity in the mode of conducting the experiments, be secured, the time over which the inquiry might be spread would become of less moment. At the same time he is sanguine enough to believe that if the object be found to be really important, there are plenty of qualified men who would be ready to give a portion of their intervals of leisure for the furtherance of an inquiry which should give a complete and satisfactory answer to questions of daily importance to our profession and to all connected with the arts of construction.

The tables appended show the breaking weights of cast-iron columns of 12 and 6 inches diameter, according to Hodgkinson and Rankine:—

Column—dia. 12 in. × 1 in. thick. Resistance to Crushing 1417 tons.			Column—6 in. dia. × 1 in. thick. Resistance to Crushing 644 tons.		
Length.	Breaking Weight.		Length.	Breaking Weight.	
	Hodgkinson.	Rankine.		Hodgkinson.	Rankine.
ft.	tons.	tons.	ft.	tons.	tons.
10	3820	987	6	1243	413
12	2808	907	8	768	342
14	2145	828	10	522	280
16	1720	753	12	384	230
18	1404	682	14	294	190
20	1171	617	16	176	158
22	997	558	18	144	132
24	860	506	20	120	112
26	752	459	22	102	96
28	663	417	24	88	83
30	589	380	26	77	72
			28	68	63
			30	60	56

DISCUSSION.

Mr. SCHÖNHEYDER said he had noticed the discrepancy of the formulæ referred to in the paper, as he had often to calculate the strength of columns, and he hoped the experiments would be made which Mr. Light suggested. In addition to those he would suggest further experiments upon columns placed in horizontal or angular positions, as those were cases that often occurred in practice. There was no doubt that the crushing load which they would bear was very much less than that which a vertical column would bear. Further experiments should be made upon columns which were loaded a little to one side of the centre, as in that case the strength was very much reduced.

Mr. HEAD said it appeared to him that the great difficulty Mr. Light seemed to be under was the testing and calculation of compressive strain chiefly in cast iron. The tensile strain he thought they had always been able to find out, having regard to the different kinds of iron; but where Mr. Light seemed to find difficulty in getting any calculations that came into practice, was chiefly in long columns of cast iron and also in wrought iron. In his (Mr. Head's) experience the compressive resistance had always been so immensely beyond what was put on material in tension, that it was very seldom necessary to calculate it, except in case of bending; and as to the actual compressive strain of a cast-iron column, founders never could cast columns so thin as a calculated column would be. If engineers could arrive at an exact compression to put on a given column, it would have to be multiplied by about six before the column could be cast. There was a very great difficulty, of course, in producing any formulæ for such columns as those referred to by Mr. Light. The experiments were very difficult with large columns, though the smaller experiments were very easy. His firm had put up a machine in London for the purpose of testing such strains, and he should be very glad to place it at the disposal of the Society if they wished to make any small experiments; but such experiments as those that Mr. Light had suggested, he (Mr. Head) thought would be impossible at present for want of proper machinery.

Mr. KIRKALDY, in reply to a question from the President, said he had tested logs of timber 20 ft. long and 13 in. square, in compression; wrought-iron girders 3 ft. deep and 26 ft. bearings, small pieces the tenth of an inch in diameter and a quarter of an inch long, in compression; steel wire, No. 22 gauge, and cement in tension. The machine afforded the opportunity of testing materials under twisting strains and all classes of

strains. He further observed that the power of his machine ranged from 10 to 1,000,000 lbs. He was proceeding with some experiments, the result of which were being tabulated systematically, and perhaps at a future day they would be published. At the present time, with few exceptions, when any great accident happened, no one ever sent to have the materials which had been used tested, although they had the means of so doing.

Mr. Olrick thought that the only difficulty in the way of making experiments on a large scale, as suggested by Mr. Light, was the want of funds. With regard to the question of strain, he (Mr. Olrick) was of opinion that, practically, the shearing force was almost identical with the tensile strain per square inch of a piece of iron.

Mr. W. Adams said he fully agreed with Mr. Light in the opinion that a very extended series of experiments was required in all branches of mechanical engineering. Having experienced the want of some definite knowledge about the strength of riveted joints, he had instructed Mr. Kirkaldy to make experiments which he was now carrying out, and, as far as they had gone, they had convinced him (Mr. Adams) that he had very imperfect ideas on the subject. He had not arrived at any definite conclusion yet, but his object was to find what pitch of riveting in the seam of a boiler would give the greatest strength at the seam. It was a very simple matter when riveting up girder plates to put them in a testing machine and ascertain what strain they would bear; but in boiler work there were other considerations than in girders, which made it more difficult. If the rivets were placed too far apart the work could not be made steam-tight. He agreed with the author of the paper, that an exhaustive series of experiments upon the strength of materials would really be invaluable to the profession.

Mr. Cargill said that, while admitting the necessity for further experiments in connexion with the subject of the paper, he did not think that any reasonable series of experiments would exactly fulfil the conditions which Mr. Light expected. He (Mr. Cargill) had heard a column in a horizontal position referred to during the discussion, but there was no such thing. A column in a horizontal position became a beam or a girder, and was attended by circumstances totally different from those which acted in crushing a column. The sum of the forces which acted upon a column might be represented by unity, and they would be a crushing strain and a bending strain; and the difficulty of drawing a distinction between a short and a long column was in determining which of those two forces exercised the greatest influence towards breaking it. Evidently the crushing strain became very dis-

proportionate to the bending strain in the case of a short column, and *vice versâ*. Looking at Mr. Hodgkinson's formula, it was evidently composed of two parts, one they might call the constant, 44·34, and the other the fractional part, which was an exemplification of the difference between practice and theory, because, if the theoretical conditions were actually fulfilled, it would be identical with that established by Euler. It appeared to him (Mr. Cargill) that in order to make the experiments complete, a different constant would be required. Supposing that the theoretical part of the formulæ remained the same, which was the fractional part, a different formula would still be wanted for every proportion of length to diameter. That would be very tedious, and involve calculations *ad infinitum*. Moreover, it would not be of much practical value, because with such a number of constants there would be liability to confusion. It would be better to take half a dozen constants, which would be of real value in practice; a formula could not be had for every case: a number of general formulæ and experiments must be taken, and allowance made for the trifling differences in practice. The manner in which the experiments were to be made, consequently, presented much difficulty. On the one hand, if they were of too limited and contracted a nature they would fail in their object. On the other hand, if they were to be very extensive it would be impossible to adequately carry them out. With care, however, that might be obviated, and a useful series of rules and formulæ might be obtained.

Mr. Schönheyder agreed with Mr. Cargill that there was no such thing as a horizontal column, but if a rod was laid horizontally and exposed to a crushing strain, he did not think that it ought to be described or calculated as a girder. For instance, he did not think that the strut of a roof composed of T iron in a sloping direction, at say say 45°, should be calculated as a girder. Certainly it was not loaded similarly to a girder, and the crushing strain by far preponderated, and it would be better to calculate it as a column, which would bring the result nearer the truth than if it was calculated as a girder.

Mr. Cargill said that a rafter occupied an intermediate position between a column and a beam, and was certainly subjected to compressive force. In a rafter there would be a transverse strain, but in roofs and bridges the struts were supposed to have no transverse strain at all, but to be simply acted upon by compressive forces in the direction of their lengths. If 'e were any, the rafter would be subjected to a bending in, but a compressive force was the only one taken into ›unt. He submitted that although it was a compressive in, it was not absolutely such a directly vertical and crushing

strain as a column had to undergo. It was modified by the angle of inclination. With a horizontal beam it was a very different thing, because, although there were compressive and tensile strains, yet, taking a plate girder for instance, and loading it till it broke, the top flange would yield by compression, but not in the direction that a column would.

Mr. ROBINS said that a bridge built by Mr. Charles Warren many years ago, had a horizontal column at the top, which took all the compressive strain. That was a case of a horizontal beam for a bridge of about 272 feet span.

Mr. SCHÖNHEYDER cited another example of what he might call a horizontal column, namely, the upper member of a girder, say of a lattice girder or a Warren girder-bridge. As a rule, the upper member was kept from deflecting upwards or downwards, by connexion with the lattice bars, but it was not always prevented from deflecting laterally in the same manner; it was simply prevented from that action by it own stiffness, and it was necessary in very long bridges to calculate the amount of that stiffness, so as to be sure that it would not deflect laterally.

Mr. SEFI said that if a formula could be arrived at which would mathematically express any strain that a column of a given length should bear, he thought that the discrepancies existing between different samples of iron would entirely vitiate calculations founded upon such a formula. A column of cast iron might be either perfect or imperfect. Taking it as perfect it might, probably, bear a strain of 30,000 lbs., but it would be easy to find another specimen equally perfect that would not bear 20,000 lbs., not through any inherent defect, but simply from being of a different class of iron. As a further illustration of horizontal struts and columns he would refer to the ordinary tie-rods which tied the cylinders and condensers of horizontal marine engines together, inasmuch as in the back stroke of the piston the entire strain of the crank would be brought upon the bearings alone if they were not otherwise supported between the cylinders and condensers.

Mr. CARGILL asked if it was a tie or a strut.

Mr. SEFI said it was a strut one way and tie the other, according to the direction of the stroke.

Mr. CARGILL said it must, of course, be made to act as a strut.

Mr. SEFI observed with reference to the riveting of steam-joints that it was usual to make all the rivets of the same diameter, either for single or double riveting. He would suggest that the rivets be placed at a pitch that would give the greatest strength, and that to make the joint steam-tight rivets of a smaller diameter should be inserted between the larger ones.

The PRESIDENT said he would adjourn the discussion to the next meeting, and in the mean time the members could examine the formulæ in Mr. Light's paper, which would enable them the better to resume the discussion. The subject of the paper was of great importance, and ought to be well discussed. The members appeared to agree that it would be advisable that a committee of the Society should be formed to further the object Mr. Light had proposed, which was to investigate the strength of materials more carefully than had hitherto been done. The difficulty, however, was to find those who would give their time in carrying out the object. It would be desirable, he thought, to find somebody who could give his entire attention to experiments of that kind, under the guidance of a committee appointed by the Society.

December 6th, 1869.

F. W. BRYANT, PRESIDENT, IN THE CHAIR.

THE NEED OF FURTHER EXPERIMENTS ON THE STRENGTH OF MATERIALS.

ADJOURNED DISCUSSION.

Mr. LIGHT, after giving a short *resumé* of the discussion at the previous meeting, with the view of recalling the points which had been noticed, observed that the example of a column given in his paper was not chosen as a suitable one for testing, but only to exemplify in a striking manner the different results obtained from the formulæ. He had taken 12 inches as a very fair diameter for a column, and his reason for taking a 30 ft. length was to bring it up to Hodgkinson's assumed limit for short columns. It appeared to him that Mr. Head's remarks would logically lead to more than he probably intended. The fair inference from them would be that as the practical strength of the columns was always so very much in excess of the required strength, and as the formulæ given in the books of reference differed so very much from one another, as he (Mr. Light) had shown, the right course would be to efface those formulæ, and to insert in their place a note to the effect that it was quite sufficient to attend to the practical thickness to which castings could be made. He could not agree with that position, because he did not believe that Mr. Head himself would say that a column 12 ft. long, 6 in. diameter, and $\frac{3}{4}$ in. thick, would not in many cases have to bear quite as much as 45 tons upon it. If the load exceeded that, as it very well might do, it became a question of great importance whether Rankine was right in putting the safe load at 45 tons, or Hodgkinson at 80 tons. As an actual case he might quote some of the columns in a design he had himself lately made, which were 15 ft. 6 in. long, 6 in.

in diameter, and $\frac{7}{8}$ in. thick, and which were to carry 39 tons each. Mr. Hodgkinson's breaking strength would be 175 tons, and therefore the safe strength would be about 44 tons. According to Rankine they would break at 148 tons, and therefore their safe strength might be taken at 37 tons. There was thus a difference of 7 tons as to the load that might safely be placed upon them, and certainly from what he knew of the accuracy with which the columns for the Exhibition of 1851 and for the Crystal Palace were designed, he had not the slightest doubt that if they could obtain the particulars of those columns they would find that there was no such very great excess in the practical casting over the theoretical calculations. The tables appended to the paper illustrated still more strongly the different results of the two formulæ in 12 in. and 6 in. columns of varying lengths. They served another purpose also—they would show the principle upon which the two formulæ differed. It would be seen that the resistance to crushing in each case showed that Hodgkinson's formula, when driven to an extreme, could not possibly be correct. Rankine's formula appeared to him to come out exceedingly well, and to be in form what would be expected of a true formula.

Mr. Cargill said that when there was so much discrepancy found between the formulæ of different engineers, there was nothing like putting them to a practical test. He had prepared two tables, Table No. I. and Table No. II., from Mr. Hodgkin-

Table I.—Solid Pillars.

No.	Ratio of D : L.	B. W. From Experiment.	B. W. From Calculation.	Difference.
1	$\frac{1}{24}$	3·21	3·27	0·06
2	$\frac{1}{20}$	3·98	3·96	0·02
3	$\frac{1}{15}$	5·02	5·14	0·12
4	$\frac{1}{8}$	7·80	7·59	0·21
5	$\frac{1}{26}$	6·96	6·97	0·01
6	$\frac{1}{20}$	9·40	9·48	0·08
7	$\frac{1}{15}$	10·84	12·07	1·23
8	$\frac{1}{13}$	11·57	13·11	1·54
9	$\frac{1}{10}$	14·29	16·13	1·84
10	$\frac{1}{20}$	14·20	15·91	1·71
11	$\frac{1}{15}$	17·97	19·55	1·58

TABLE II.—HOLLOW PILLARS.

No.	Ratio of D : L.	B. W. From Experiment.	B. W. From Calculation.	Difference.
1	$\frac{1}{24}$	15·04	14·43	0·61
2	$\frac{1}{21}$	15·76	16·30	0·54
3	$\frac{1}{22}$	13·93	12·84	1·09
4	$\frac{1}{20}$	13·56	13·52	0·04
5	$\frac{1}{17}$	18·64	17·91	0·73
6	$\frac{1}{18}$	12·11	13·14	1·03
7	$\frac{1}{15}$	11·39	11·69	0·30
8	$\frac{1}{14}$	11·21	11·73	0·52
9	$\frac{1}{13}$	11·93	12·22	0·29
10	$\frac{1}{14}$	12·11	13·78	1·67
11	$\frac{1}{12}$	16.65	17·97	1·32
12	$\frac{1}{8}$	15·20	14·13	1.07

son's work, giving first of all the proportion of the diameter to the length of the column, and then the breaking weight by actual experiment—by breaking the column, and then the breaking weight calculated from his formula. Of course, the closer the calculation agreed with the absolute experiment, the better for the truth of the calculation. What were called short columns were those in which the maximum proportion of the length to the diameter was 26. In fact they just bordered upon a line of demarcation between a short and a long column. The greatest difference between theory and actual experiment was two tons. The size of the column was about 5 or 6 ft. He was not certain how the diameter varied, but it was not so much the length, but the proportion between the length and the diameter which constituted the long or short column. Some of those agreed almost identically, the difference was so low as 0·01 of a ton. He had prepared the table to show that Hodgkinson's formula must have been based upon some truth, because his theoretical calculations coincided very closely with the actual results of his experiments. The author of the paper asked whether a column 30 ft. long and 12 in. diameter should be taken as a short column or a long one. He, himself, should take it as a long column, and treat it as such, but of course that was open to opinion. There was a formula which Mr. Light had quoted from Molesworth's Pocket-book, although he

could not say whose formula it was. He (Mr. Cargill) might mention that it was known as Professor Gordon's Formula, and was as follows:

$$W = \frac{S \times 36}{1 + \frac{L^2}{400 \times D^2}}$$

With a table of sectional areas or of circles the formula as altered below would be found easier to calculate from, as the square of the length in inches, which was a troublesome calculation, was dispensed with, and he had put N there as the ratio of the length to the diameter. By substituting for the length its ratio to the diameter and calling it N, the formula became:

$$W = \frac{14400 \times S}{400 + N^2} \quad . \quad . \quad (1)$$

$$W = \frac{11309\ D^2}{400 + N^2} \quad . \quad . \quad (2)$$

It of course became very ready for calculation where N happened to be a whole number, and simplified the calculation very much. Without a table of areas, the second formula was easier still, as it was only necessary to calculate the square of the diameter. It was to be noticed that if Gordon's formula erred it did so on the safe side. It made the breaking weight less than Hodgkinson's. Referring to the discrepancies between the two formulæ, he observed that he did not allude to discrepancies where different exponents were taken, because according to the index taken different results would be obtained. The reason there was so much difference in those results was that the experiments were made with columns of different lengths, and made from different irons. That, of course, would show discrepancies which must be reconciled by taking the mean or average. There was another difference to which he would refer. Mr. Light gave in one instance a breaking weight of, in round numbers, 4100 tons, and Gordon's formula made it 955 tons. He (Mr. Cargill) thought that Mr. Gordon's formula was not at all applicable to the case. He should take it, that the value of N was fifteen times the proportion of the length to the diameter.
uld calculate it as a short column, and Gordon's formula
t applicable to short columns, only to long ones; therefore

that result of 955 tons ought to be taken out of consideration altogether. It was not to be supposed that the late Mr. Hodgkinson, who was an expert experimentalist, and one of the ablest mathematicians of the day, had committed such an error as to give a formula which would make the breaking weight four times what it ought to be. He was not aware that any columns which had been calculated on that formula had given way at any time.

Mr. LIGHT said, with respect to Mr. Cargill's two tables, he understood that the columns experimented upon were all at, or about, the proportion at which Mr. Hodgkinson assumed that the change from a long to a short column occurred. Was that so?

Mr. CARGILL said he thought the proportion was 1 to 26.

Mr. LIGHT said he was certainly surprised at so close an approximation between the calculated and actual strengths as was shown in those cases. He could say no more then, as no information had been given as to the mode in which the experiments were made. Reverting, however, to his own tables he thought that when it was seen that the crushing resistance of a column, looked upon simply as a mass of metal without any regard to its proportion of length to diameter, was according to the best authorities about 644 tons, and that a formula for the same column at 6 ft. long (which surely was not pushing it very much to the extreme), made its breaking weight 1243 tons, there must be something seriously wrong in the formula. It was to be borne in mind at what period Mr. Hodgkinson's experiments were made, and what was the state of scientific knowledge at that time, how little the best men then knew about such things at all, and he thought there could be no reflection upon Mr. Hodgkinson if men of the present day differed somewhat from his deductions. He did not think that Mr. Cargill had succeeded in disproving the calculations in the paper, and his (Mr. Cargill's) remark that columns had not broken which were calculated by the recognised formulæ was certainly at most a negative objection, because it was quite clear that when they allowed a margin of safety of 4 or 5, columns might be very nearly on the breaking point for years without much being known about the matter. In reply to Mr. Cargill's observation as to the accuracy of the lengths which he (Mr. Light) had given as the extreme lengths upon which Mr. Hodgkinson experimented, they ought to be correct, for they were taken from his own work. He had hoped that the case of wrought-iron struts would be alluded to in the discussion, because it was rather an important one. It was, of course, usual to take the section of the strut at

3 or 4 tons per inch, but still it would be well to know if there were any actual experiments in testing machines upon such struts, to ascertain how far the usual mode of calculation was simply a rough way of getting at the result or could be relied upon.

The PRESIDENT said he thought engineers had ample formulæ for use, but it would be advisable that all those who were carrying on works should make experiments on the materials they used. They had an excellent opportunity of doing so with Mr. Kirkaldy's machine, which would prove of great value to all requiring it.

December 6th, 1869.

F. W. BRYANT, PRESIDENT, IN THE CHAIR.

APPARATUS FOR MEASURING THE VELOCITY OF SHIPS.

BY VAUGHAN PENDRED.

No subject has of late years excited more continuous attention than the speed of the ships of Her Majesty's navy. This fact would alone be sufficient justification for bringing before this Society the questions treated of in the following paper, but, in addition, the speed of the fine steamers and splendid clipper ships of our mercantile marine constitutes a question of great interest, and matter, of which, as a nation, we are justly proud. For some time past the British Association has been engaged, through a committee of able men, in investigating the laws which decide the best forms for speed of steam-ships, and the importance of such an inquiry no one can dispute. It should be clear to every one that accurate methods of determining the velocity of ships are essentially necessary to the solution of questions connected with the conditions under which maximum efficiency, as regards speed, is to be obtained. The importance of possessing a thoroughly good method of ascertaining the speed of a ship sailing or steaming will become apparent from even a superficial knowledge of the system of navigation now generally adopted by seamen all over the civilised world. But apart from all these considerations, the author believes that yet another exists which will commend this paper to the attention of engineers. He will have occasion to describe a certain form of speed-measuring apparatus which throws a novel light on certain questions connected with the laws of induced currents, which laws have already afforded matter for able papers and discussions read and carried on in this hall, and one of his main objects in reading this paper has been to bring those questions up once more for discussion from a new point of view.

Before proceeding to describe any of the devices which have from time to time been invented for measuring the velocity of ships, it will be well to define what is meant by the velocity or speed of a ship, and to state plainly that, with one exception to be presently defined, only a single method of determining the speed of a ship with accuracy has any existence.

By the phrase "speed of ship" either of two things may be meant. First, her velocity as regards the water on which she floats partially immersed; secondly, her velocity as measured by the time it takes to proceed from port to port or from point to point. These are two utterly different things, and it is well that the difference should be carefully kept in sight.

In order to estimate the true velocity of a ship due, say, to a certain power of engines, we must regard the water as being absolutely at rest with regard to the vessel; for it is obvious that if the water in which she is partially immersed has a proper motion of its own, that motion will affect the absolute velocity of the ship through space, accelerating or retarding her according as the motion of the water is with her or against her. Thus, a vessel going on the measured mile with a tide of three knots an hour under her will have an absolute speed three knots an hour faster than her true still-water speed, when running with the tide, and three knots an hour less than her true still-water speed, when running against it. In order to eliminate this factor, ships on the measured mile run three times up and three times down the mile. The average velocity of all six runs found by the mean of means is then taken to represent her true speed, it being assumed that she will be as much accelerated during three of the runs as she is retarded through the other three. But this assumption is not necessarily well founded, because it is possible so to arrange the time of taking the runs with regard to the tide, that only two of the runs out of the six shall be against the tide, three of the runs with it, and one at slack water, or even on the turn. By this means the velocity of a ship can be made to appear greater than it really is. Honestly conducted, however, the measured mile trial constitutes the only means now used in Her Majesty's navy of ascertaining the true speed of a ship, and this is one of the two methods referred to as exact in the beginning of this paper. Measured mile trials, however, are full of defects, well known, and resulting in the fact that the speed of a ship on the measured mile is always apparently greater than her speed at sea.

Of late years the system of six hours' trial has been introduced. A ship runs down Channel for three hours, and then back again to her port of departure, say Portsmouth, for three hours. The distance run in the time is regarded as deciding

her speed. But, in the first place, unless much care is taken it is quite possible to have a strong tide telling either against or for the ship the whole time; and, in the second, no accurate means of determining the distance run exists. It is of course easy to say that from point to point (taking landmarks as a basis) the distance is by chart so much; but a bad steersman, or the influence of the wind or sea, may cause a ship to go over much more water than that indicated by the position of landmarks on the chart; for if the ship took a sinuous course it is obvious that her true speed could only be ascertained be measuring the circumferential length of the curves traversed. But the chart assumes that the ship traversed only the chords of these curves; very frequently, as a consequence, the distance is measured by the number of revolutions made by the screw; slip being deduced from the measured mile trial. Nothing can be less scientific. The six hours' trial trip system is therefore eminently unsatisfactory, and it remains a fact that no means of measuring the absolute velocity of a ship with minute accuracy is now in general use.

But it is not during trial trips alone, or even principally, that it is expedient to know the exact speed at which a ship is moving from point to point of the earth's surface. To the practical navigator the means of obtaining a good general idea at least of the speed at which he is sailing or steaming are absolute necessities. The author is not about to take up the time of this meeting by any dissertation on the science of navigation. He will proceed at once to assume that the existence of a speed measurer on board every ship is recognised as essential to proper navigation, and go on to describe such apparatus for measuring the velocity of ships as appear to deserve attention.

The apparatus in most general use for this purpose is the common log. This is made of wood, in the form of a sector of a circle, loaded with lead at its circumference, so that if left to itself it swims upright in the water with the apex at the surface. The log is usually 5 in. or 6 in. long and $\frac{1}{2}$ in. thick. It is attached to the log line by two cords, one of which is fixed at the apex of the sector permanently; the other passes through a hole near the circumference, and precisely in the middle of the width of the log. It is secured by a wooden peg in such a way that when the log is exposed to the strain of hauling in, the peg comes out, the lower part of the log is released, and it floats flat, or nearly so, on the water, offering little resistance to being hauled on board. The line attached to the log is marked at certain equal distances with knots, which are again subdivided into tenths, called fathoms. It is wound on a reel, and when it is wished to find the ship's rate the log is thrown overboard, and

a quantity of line allowed to run off the reel that the log may float clear of the ship's wake. This part of the line is called stray line. It varies in length from ten to twenty fathoms, and is marked with a piece of white bunting. When the bunting passes over the quarter the officer calls out, "**Turn.**" A half-minute glass is then turned, and directly the glass is run out the assistant calls out, "Stop." The line is then checked, and the pull of the water on the log causes the peg before noticed to come out, and the log lying flat on the water is hauled in. The operation is called heaving the log. The log glass runs out—in theory—in half a minute, or the $\frac{1}{120}$th part of an hour, and if the divisions between the knots be the $\frac{1}{120}$th part of a nautical mile, it is apparent that if one division be drawn out in half a minute the speed of the ship is a mile an hour; if two, then her speed is two miles an hour, and so on. For each knot passed overboard in half a minute the ship is going one nautical mile an hour, and this is the origin of the word "knot," as will be easily seen. The length of a knot is found thus:—Circumference of earth=360 deg. and 360 × 60 nautical miles=21,600. In practice the length of the nautical miles is usually taken as 6080 ft. The distance between the knots should be 50 ft. $7\frac{3}{8}$ in.; it is really 47 ft. to compensate for errors due to the log coming home.

It is difficult to conceive a more rude method of measurement than that just described. The rate at which the sand runs through the glass is affected not only by the weather, but by the motion of the ship. The length of the line is affected by its repeated immersions, and last, but not least, the hold of the log on the water at slow speed is so small that unless the line is payed out by hand an error of a knot or two an hour may be easily caused by the mere friction of the reel, however carefully that is mounted. As a result we find in every treatise on navigation a special warning to the student not to depend absolutely on the log, and formularised directions as to the means of eliminating such errors as can be got rid of. The author cannot do better here than refer such of his hearers as wish to investigate this matter further to Merrifield and Evers' very able treatise on "Navigation and Nautical Astronomy," published by Longmans in 1868.

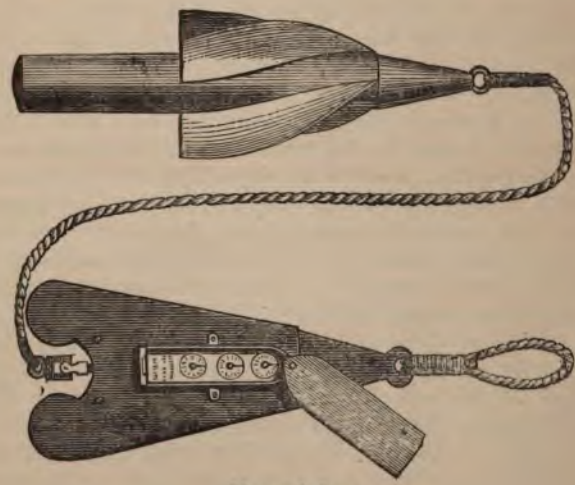
The first mention of any form of log with which the author is familiar, occurs in an account of an East India voyage by Purchas, dated 1607. Gunter described the log in 1623, and it is noticed in all succeeding works on navigation. From 1623 to the present day the common log has scarcely undergone any modification. Its defects, however, became apparent very early,

and numerous attempts were made, not to improve it—a hopeless task—but to invent something better.

Very far back indeed in the history of scientific discovery we find evidence that it was known that a fluid current impinging on inclined vanes would cause rotation. No one knows who invented the windmill for example; it is pre-historic in a sense. It was also discovered that by moving vanes inclined to their axes through a body of fluid at rest, rotation would be set up in the vanes by the creation of an artificial current, and it was also understood that the velocity of rotation might be made to bear a fixed relation to the velocity of the current on the one hand, or the velocity with which the vanes were drawn through the resisting medium on the other. Here, then, was the germ of an idea for a log. Attach a screw to a rod and allow it to tow behind a ship in such a way that one end of the rod could—impelled by the screw—rotate inboard, then, assuming the number of turns made by the screw in a minute to bear a definite relation to the velocity of forward progression of the ship, nothing was easier than to design a counter, which, attached to the rotating rod, would show from time to time on a dial figures from which the speed of the ship could be calculated. This is the principle which has been involved in a number of inventions too numerous to particularise. Patents for improvements in logs were taken out anterior to the year 1703; but as no descriptions are filed with the patents, it is impossible to say in what the patent consisted. In the "Philosophical Transactions" for 1725, however, Mr. Henry de Saumerez describes machinery for measuring ships' way better than the common log. This consisted of a Y-piece capable of rotating on the straight tail of the Y, the arms of which were each fitted with a vane or screw blade. The Y was fixed to a rope and towed astern. The rope was caused to rotate by the action of the water on the vanes, and, to use Smeaton's words in describing the apparatus, "the value of a certain number of turns once found by experiment, they are easily reducible into leagues, &c. The only difficulty is, will the same number of turns be always made in passing through the same distance?" Smeaton commenced experimenting with this apparatus in May, 1751, on the Serpentine. The results of his experiments led him to the conclusion that the velocity with which the ship moved forward made no material difference in the velocity of rotation—or, at least, that the difference would be less than that arising from other causes. The first log was too big—being 27 in. long, and the vanes 8 in. long by 4 in. wide. Smeaton then constructed an improved log, consisting of a plate of thin sheet brass, oval, 2½ in. wide and

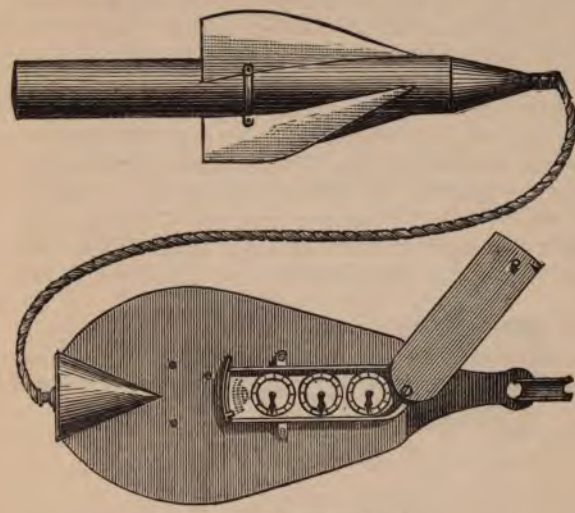
10 in. long, and delicately mounted on a spindle only one-twentieth of an inch in diameter, rotating in agate holes. Very ingenious arrangements were introduced to save this delicate apparatus from being broken, on which it is not necessary to dwell. With this apparatus a great number of experiments were made at the Nore, from which it became apparent that the slower the speed of the ship the greater were the number of revolutions made by the log—exactly the reverse of what might have been expected. As an instance of the extreme irregularity of the results, one experiment may be cited in which the speed of the vessel or sailing ship was measured both by the common log and the screw log; 375 yards of log line were run out in periods varying from 45 seconds to 135 seconds, the revolutions of the screw on the same occasions being 62 to 83 seconds. Here we have 62 turns made in 45 seconds, and 83 turns in 135 seconds. Two screw logs, however, tested over a long time together, gave, one, 86·716 revolutions, while the other made 88·184 revolutions. On the whole Smeaton concluded that at moderate speed it was possible to use a screw to register the velocity of a ship with some approach to accuracy.

In 1792, Gower patented a screw log, and several others followed his lead up to the year 1834, when Mr. Massey patented his well known log, modifications of which are in considerable favour now, especially in America, under the general title of Massey's patent log. In 1836 he secured another patent, a



MASSEY'S LOG.

third in 1844, and a fourth in 1848. As Massey's is one of the few screw logs which have competed successfully with the ordinary log, it may be well to notice it at some length. In one form it consists of two portions, the rotator and the register.



MASSEY'S IMPROVED LOG.

The first consists of a short length of light brass tube, to which spiral vanes are soldered. This rotator will nearly float in water, owing to the lightness of the central tube. The register is simply a triangular box fitted with vanes to prevent rotation. It con-

MASSEY'S FRICTIONLESS LOG.

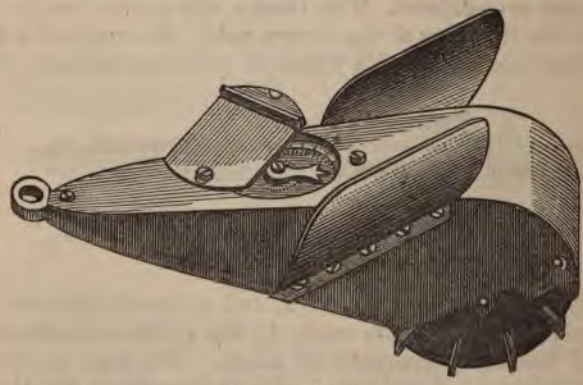
tains a train of simple wheelwork, and a set of dials on which the number of revolutions made by the rotator are recorded, in terms of knots and fractions of knots. The gear is so simple that it requires no description. An endless screw sets the train in motion, the screw being itself connected with the rotator by a short length of Manilla rope. In using this log the hands are set at zero, and the log hove overboard, attached to a line ten or twenty fathoms long. The time of heaving is noted; at the

end of a certain number of minutes or hours, the log is hauled on board, and the number of knots run since it was heaved noted. The result divided by the time gives the speed. The log is very different in many ways from those originally patented by Massey. In his first arrangement the log was anchored to the heel of the stern-post; it consisted simply of a rotator driving a recording apparatus, placed inboard, by a set of turned rods. In a subsequent patent he claimed the use of electricity for registering inboard the rotations of a log floating astern. Nothing would be gained by describing at length arrangements which never came into actual use. The Massey frictionless log is favourably spoken of. It will be seen that in it the rotator and recorder are contained in one.

Another very neat speed-measuring apparatus is that known as Walker's harpoon log. Its characteristics will be taken in at

WALKER'S HARPOON LOG.

a glance—it is Massey's log in a compact form, the recording arrangements being nearly the same. This much resembles Massey's frictionless log. In Friend's log, a paddle-wheel is used



FRIEND'S LOG.

to drive the recording apparatus instead of a screw. Other forms of screw log have been patented, but it would be mere waste of time to describe them, as they are all identical in principle.

It will be seen that the value of any screw log must depend on the uniformity with which a given number of revolutions are made in running a given distance, say one knot, at all speeds likely to be attained by a ship, and under all conditions of weather. Tested by this rule the screw log is found wanting. In the first place, the number of revolutions made in traversing a given length of water will depend on the resistance of the recording gear, and the angle at which the vanes are set—in other words, on the pitch; besides this, variations in the depth of immersion will affect the rate of rotation. Now, it is simply impossible that the gearing, light and small as it is, should always remain in perfect order, and a little neglect in the lubrication will materially increase the resistance. Very little rough usage will suffice to bend and distort the vanes, and render the instruments quite unreliable. They are, indeed, delicate pieces of apparatus, and require careful handling; but, apart from these considerations, a grand defect lies at the root of the principle of these screw logs. It has never yet been proved that precisely the same number of revolutions will be made in running a given distance at all speeds. Far from this being the case, Smeaton's experiments already quoted, proved the contrary. At very high speed and in rough weather he noticed that the log he used was constantly dragged out of the water, flying from one wave to another across the hollows. Upon the whole there is reason to believe that a screw log is capable of approximately measuring the speed of ships, and the slower the speed and the smoother the water the more reliable will be its indications; but it must not be forgotten that at velocities of less than three knots an hour the indications of the screw log are totally unreliable. It is no wonder, then, that the common log remains in favour with all shipowners, and that the use of the screw log is to a great extent exceptional.

It is further to be noticed that the screw log will not show immediately what the speed of the ship is; both that and the common log measure the velocity of the ship by the time taken to traverse a given distance, and time must, therefore, elapse between the heaving of the log and its reading before the rate of speed can be calculated. In yacht races this is very objectionable, and it is at all times more or less inconvenient. A log which would show at a glance and instantaneously, not only the actual speed, but every variation in the speed of the ship with absolute accuracy at all speeds, would obviously possess important advantages over all others. With the common and screw log we must first measure a given distance run before we can determine the speed. In fact, they are not speed measurers, but distance measurers; and they are more or less imperfect

measurers of distance. What is really wanted is not a distance measurer but a speed measurer. In using the former we first ascertain the distance run and then the speed; by using the latter we first ascertain the speed and then the distance run. It may be asserted that these things are identical; but the author submits that they are totally different. In the first place, all the distance measurers in use are inaccurate, but a speed measurer may be made which shall be absolutely accurate; and, in the second place, no distance measurer can tell in a moment what is the effect of setting or taking in of a particular sail, or of trimming a ship in a particular way; but a speed measurer will tell both, or either, the moment the sail is set, or the trim altered.

It now becomes the author's duty to describe apparatus by which speed may be measured without regard to distance run; and in so doing it is his good fortune to call the attention of the Society to one of the most beautiful contrivances ever produced, to one of the most exquisite applications of a recondite law of nature to a practical purpose ever effected, to an invention which has successfully borne the test of over twenty years' hard service, to a natural phenomenon regarding the induced current, and to a yet unnoticed law of the action of that current, that can hardly fail to prove eminently interesting. Before describing this apparatus it will be necessary to call attention to one or two facts connected with fluid pressure.

When a ship is advancing through the water it is evident that her bows are exposed to a greater pressure, due to the resistance of the water, than her stern. If, then, a pipe, say half an inch in diameter, were fitted in her hull, in such a way that one end opened in the cut-water below the water-line, while the other was turned up towards the deck above the water-line, it is obvious that as she advanced through the sea the water contained in the horizontal portion of the pipe would be driven inwards, and would rise in the vertical leg above the level of the water-line till it balanced by the gravity of the vertical column the outer pressure. It is also clear that the pressure would depend on and vary with the rate of progression of the ship, and that, as a consequence, the speed of the ship at any moment would be denoted by the height of the column in the vertical log of the tube. This constitutes "Peto's tube." Here, then, we have all the elements of a perfect continuous log. One individual, whose name the author forgets, was, indeed, so delighted with the idea that he patented a log consisting of a small pipe laid along the keel, opening in the cut-water with a trumpet mouth, and running aft to beneath the captain's cabin, into which it rose and indicated speed by a glass tube fixed against the bulkhead. A

consideration will show that this apparatus was abso-
rthless. In the first place, at considerable velocities,
n of water in the cabin would stand some 12 ft. higher
level of the sea; and no cabins will admit of a gauge
ed the range in which is anything like 12 ft. But, apart
is, the water in the tube, owing to the position of the
s exposed to the influence of heavy seas striking the ship's
ith a force quite competent to send the water up double
igh; and, in the second place, as the height of the column
vertical leg would be influenced by the draught of water
ship, the indications of the gauge would vary from day
y and from hour to hour, especially in steamers whose
ght decreases as her coals are consumed.

n the 20th of December, 1849, the Reverend Edward Lyon
thon patented an application of this principle, and in June
the year following a second improvement on it, which supplied
naval world with an apparatus perfect in theory and practice
r measuring the speed of ships.

This apparatus consists essentially of a small tube, called a
way" tube, passed through the keel of the ship at or about
midships, for a distance of a few inches from the keel. A small
aperture is made in one side of this tube. The tube is carried
up into the captain's cabin, or any other convenient place; there
it is turned over and united to one leg of a syphon tube inverted,
and containing mercury. A motion of about 13½ in. in the
column of water will cause a motion of about 1 in. in the column of
mercury, and the magnitude of the gauge range is thus brought
within reasonable limits. When the ship goes ahead the water
forces its way into the tube, and affects the gauge, marking the
speed of the ship as before explained; and this arrangement
has these two advantages over the first described—that the
gauge is brought down to a reasonable length, and that the
waves do not interfere with its action; but it is still open to
the defect that it is affected by the draught of water and the
rolling of the ship.

The foregoing description conveys nothing more than a popular
idea of Berthon's log in its crudest form. In this shape it was
fitted to a fast Channel steamer, and made the subject of a vast
number of experiments. Let us see with what result. The
first thing to be done was to eliminate the influence of the ship's
draught. Now it occurred to Mr. Berthon that if, the aper-
ture in the pipe being turned ahead, pressure was exerted on
the column of water within, which made it rise, by turning the
aperture astern there would be a negative pressure, and the
column would fall. Then between these two positions must lie
some negative position in which the column would neither rise

nor fall. If, then, the "way" pipe was so fitted that it could rotate on its axis, by turning it till the side aperture was in the neutral position, the precise draught of the ship would be shown inside. The index or scale would be set to this, and then on the pipe being returned to bring the side aperture ahead, a correct reading would be at once obtained of the speed of ship. Mr. Berthon very naturally concluded that the neutral position would be that in which the aperture was turned exactly at right angles to the keel of the ship; how far this was verified by experiment we shall see presently.

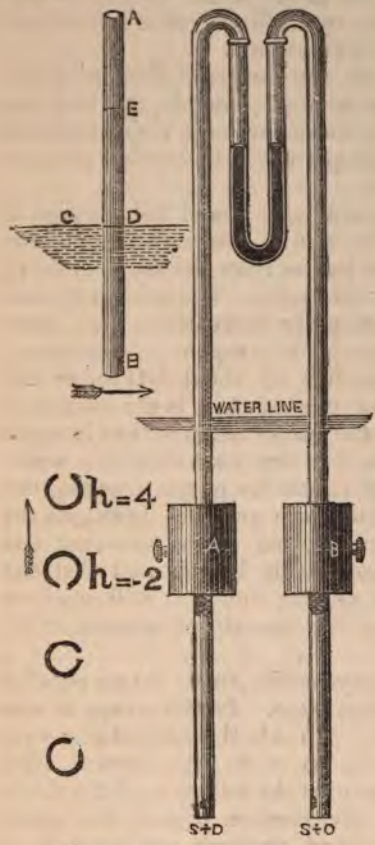
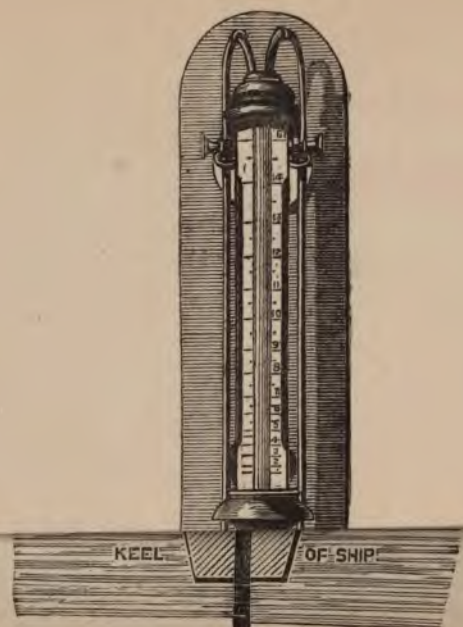


DIAGRAM OF BERTHON'S LOG.

Before going further it will be necessary to investigate the mathematical principles which regulate the ascent of the water in the tube. Let A B in the diagram represent a tube, open at the top and closed below, with an aperture at B, near the lower end, and plunged into a fluid of which the surface is C D. When at rest the fluid, disregarding capillary attraction, will stand at the same height, C D, within the tube. But if the tube be moved forward in the direction of the arrow the fluid will rise to the same height E, such that the column E C D becomes the measure of the velocity. Let h be this column. Then taking the value of h from the well-known formulæ, $V = \sqrt{2gh}$, and $h = \frac{V^2}{2g}$; and in practice this law is found to apply with strict accuracy. Done into a simple rule, the height of the column of water in the tube above the level of the fluid in which its lower end is immersed will always equal the square of the velocity of the ship in feet per second, divided by 64. Thus let us suppose that a vessel is moving at the rate of 16 ft. in a second, or nearly



BERTHON'S PERPETUAL LOG.

To face 227.]

en knots an hour, we find that the column would stand 4 ft. above the level of the sea outside the ship, for $16 \times 16 \div 64 = 4$. If the ship move at twice this velocity the column would have a height of 16 ft. for $32 \times 32 \div 64 = 16$. In the same way, by multiplying the height of the column by 64, and extracting the square root of the product, we get the speed. The author feels that he almost owes his hearers an apology for putting such well-known formulæ into words. His excuse is that it serves to impress facts on the memory which are otherwise likely to be forgotten when conveyed in a single statement heard, not read, once.

Mr. Berthon, in his endeavour to ascertain the neutral position for the aperture in his way tube before referred to, unexpectedly obtained some very startling results, and these results, invariably repeating themselves through some thousands of experiments, established the following law which has never been stated in any text-book of natural philosophy, and deserves the serious attention of all those interested in the action of induced currents. The law may be briefly stated thus:—

Let the circles *a*, *b*, *c*, *d*, denote different positions of the aperture in the way tube, and assume the speed to be ten knots an hour. Then in *a*, $h = 4$ in. of mercury; in *b*, $h = 2$ in. of mercury; in *c*, $h = 6$ in.; and in *d*, $h = 0$, or zero.

In other words, with the aperture directly ahead the column of mercury actuated by the column of water stands 4 in. above zero. When the aperture is turned directly astern the column of mercury falls 2 in. below zero, or 6 in. lower than when the tube is in the first position. When the aperture is turned to the side, that is to say, into just the position where we would expect to find the motion of the ship powerless to affect the height of the column of mercury, there is absolutely the maximum effect produced, the column being lowered not less than 6 in. below zero, or 10 in. lower than when the aperture is right ahead. But the most extraordinary circumstance of all is that the zero angle is apparently constant for all circular tubes, and all shapes of aperture and all speeds, being invariably 41° 30′.

Once in possession of these important facts, Mr. Berthon lost no time in turning them to account. It was evident that by using two tubes—one always in the neutral position, and the other with the aperture pointing right ahead—it would become possible to eliminate the influence of the ship's draught altogether. The diagram shows the arrangement most convenient in practice, with this difference—that the two tubes A B are here shown for the sake of clearness removed to some distance from each other, whereas in practice they are placed close to each other, forming one

compound tube, as in the engraving of the complete log; A and B are two air vessels, into which the water, on filling the instrument, is allowed to rise half way. Above the air vessels are air tubes, communicating, as shown, with an inverted syphon half filled with mercury. Now, it is obvious that this arrangement is absolutely independent of draught, for suppose the ship to be suddenly sunk ten feet deeper than she was the moment before, the water rises to the same height in A and B, compressing the air in the chambers, and above the mercury, indeed; but the pressure on the surface of the mercury in each leg will remain identical. All forces, indeed, except that dependent on the resistance due to horizontal motion, act equally upon both sides of the column of mercury. But the aperture in A is turned forward, while that of B is placed in the zero position 41° 30′. Let us call all the disturbing forces statical and lump them together by a great S, and the force due to the ship's motion forward "dynamical D;" then we have S both in A and B, and B exerting an equal force at all times; but when the ship moves ahead we have in A, S + D, but in B we have only S + 0, therefore the mercury will rise in one leg and fall in the other, as already explained.

The mode of fitting this log to a ship is exceedingly simple. A small hole is bored through the keel, or near it; this is lined with a brass tube having at the top a stop-cock and a small stuffing-box. The way tube of the log is passed through the stuffing-box, which is made water-tight around it, the stop-cock meanwhile remaining shut. As soon as the stuffing-box is all right the plug is turned, and the way tube passed down through the aperture in the plug till it projects 7 in. or 8 in. below the ship's bottom. A couple of composition tubes are then laid along the sides of the ship, like gas pipes, to the cabin, &c., and connected by small pieces of flexible pipe to the mercury gauge, hung in gimbals like a barometer, and occupying no more room. On this the speed of the ship can be read at any moment. Should the projecting end of the way tube be broken off, it can be repaired again in ten minutes at a cost of a couple of shillings, without admitting a single drop of water into the ship. In practice the holes in the way tubes are made very small, and the tubes themselves filled with sponge to exclude sand and dirt.

The author has now brought before this Society nearly all the information he thinks worth recording about logs. If the subject is deemed sufficiently interesting to evoke a good discussion he will rest well content with his labours.

DISCUSSION.

The Reverend Mr. BERTHON said with regard to the very curious results obtained by placing the aperture of the tube in different positions, some of the most remarkable circumstances occurred at sea in the South-Western steamer *Courier*. On one occasion he inserted a tube of considerable size in the ship's bottom through which it protruded about 8 in. There were a few tons of bilge water in the bottom of the ship, and this tube sucked all the water out. Ships might therefore be cleared of water in that way if only sufficient speed could be obtained. Several objections were brought against his log. It was said that the temperature would affect it, but it did not, for there were two air vessels, and whatever expansion of air there might be from temperature in the one, was met exactly by the same expansion in the other. The inclination of the ship would affect the log were it only single, but inasmuch as there were two tubes acting together, that disturbing cause was equally eliminated, and as the mercurial equivalent for the column of water was swung on a gimbal there was nothing to interfere with the true vertical column, which was the true indicator of speed. He (Mr. Berthon) might further mention that when a ship was making any leeway, the apparatus pointed it out most exactly; because if the aperture was turned towards the ship's bow in the direction of the keel, and if she was moving precisely in that direction, the mercury in the indicator would be seen standing at the highest point; but if the ship drifted a little to leeward, the tube was turned a little on to her port bow, and the mercury went up until the line of motion was arrived at, and the index pointed out exactly what the leeway was.

Admiral HALSTEAD said an officer in command of a ship ought to know what that ship was capable of doing, but without the log and the accompanying index it was not possible to know whether the ship was doing the utmost of which she was capable or not. With the log, however, a perfect and accurate result was obtained. The matter depended upon the recognition of the indications which the log gave by showing an increased or retarded action due to the variations of trim. By those means variations of trim were ascertained, and thus by very simple means the ship's best trim was ultimately arrived at. In the *Dauntless*, whether at high or low draught, whether steaming or sailing only, or steaming and sailing together, the trim of the ship was always settled, not as a question of mere observation, but as a matter of absolute fact, ascertained by the recording instrument. There was at first a difference between

the action of the log when the vessel was under steam and when she was under sail. What was true under sail became false under steam, for the log appeared to be affected by the currents by which the screw was supplied with its water, the log being placed aft in the ship about 40 feet in advance of the screw. The ship was evidently interfered with by the action of the screw in drawing its supply of water. The result of that was that when the ship came into harbour everything was put forward 18 inches, and there was then no alteration, whether the ship was under sail or steam.

December 20th, 1869.

F. W. BRYANT, PRESIDENT, IN THE CHAIR.

APPARATUS FOR MEASURING THE VELOCITY OF SHIPS.

ADJOURNED DISCUSSION.

Mr. OLRICK opened the discussion by observing that when the column was in the position in the tube, shown by the drawing, it altered exactly as the square of the velocity of the speed with which it passed through. Therefore, if the height of the column of mercury was four inches, that would equal four feet of water, which would equal 16 feet velocity, or 12 miles an hour. The reason why the water—as mentioned at the previous meeting—instead of being forced up the tube, was drawn down it to minus two inches compared with the zero point, doubtless was because there was a convergence of the two streams which came close behind the tube, and the two consequently formed an induced current which would draw the water out of the tube.

Mr. CARGILL said he understood it to have been stated at the last meeting that the apparatus gave no indication of a current—that was to say, a positive indication of it—but that negatively it could be inferred that there was a current. The apparatus showed when the ship drifted to leeward, and if the leeway was produced by a current, consequently, by inference, the existence of a current would be proved. It occurred to him that if a ship was sailing with a current in its own direction, it was quite

evident that although a speed would be indicated it would not be the actual speed of the ship. Moreover, if there was a current in the opposite direction it would afford no evidence of the actual speed of the ship in calm water. It would therefore appear that the apparatus did not suit all purposes.

Mr. Rigg said that he had made a diagram which he thought would show several points connected with the subject of the paper just read. Two kinds of apparatus had been brought forward—the log and the clitograph. One principle was common to both those contrivances, namely, the method of altering the range of the indicating column. In the log a very variable column was reduced into moderate dimensions by taking advantage of the high specific gravity of mercury, and in the clitograph a minute difference in the mercury column was magnified by reversing the same ingenious process.

Considering the many purposes for which that description of log might be used, it seemed remarkable that it should have met with such very limited adoption. For example, it might be very serviceable, not only for ascertaining the velocity of a ship relatively to the water in which it moved, but for measuring the skin resistances by placing the orifices at different distances from the hull of the ship while its velocity remained unchanged. It would be interesting to know if any such experiment had been made, for it seemed almost to suggest itself naturally. The question of currents driven backwards by the screw possessed equal interest with that of the currents drawn forward by the ship, and he had tried many experiments to ascertain the direction and pressure of those reverse currents. In his experiments a kind of vane was used, but such an arrangement as the log described in Mr. Pendred's paper would have been more convenient in some respects, and perhaps more accurate.

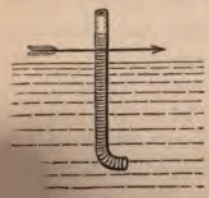


Fig. 1.

The original form of the apparatus was the Peto tube, as shown in Fig. 1. It was used to ascertain the velocity of river currents, and was the means of proving the important fact that the current ran more quickly at the surface than at any other part of the stream. That was quite opposed to the previous theories on the subject. The elevation of $2\frac{1}{4}$ inches in the tube, hown in Fig. 1, represented a velocity of one foot per second the stream, Several experiments were made with those es to ascer the pressure of currents against the edges of d bodi ferent thicknesses. The experiments were de b at, and the prisms used were each 12·8 in.

square, but of different thicknesses. The first, A, Fig. 2, being 3 in. thick, B, Fig. 3, a cube, 12·8 in. thick, and C, Fig. 4, a

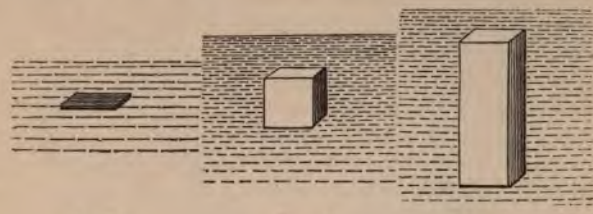


FIG. 2. FIG. 3. FIG. 4.

prism 3 times the length of the base, 38·4 in. long. In all cases the velocity of the current was 38·18 in. per second, and it was found that the pressure upon the anterior faces was always equivalent to a head of 14·28 in., and 625 trials were made to prove that. On the posterior side the pressures were very different, averaging as follows :

A. Flat plate, mean pressure	=	8·04	in.
B. Cube „ „ „	=	3·24	„
C. Prism „ „ „	=	1·80	„

These experiments seemed to prove that the form of tube would have considerable influence on the pressures of the currents, and might also affect the neutral point.

The circular form of tube would be almost that shape which offered the least resistance to water, as in the piers of bridges. The ellipse, shewn in Fig. 5, was the true form for least resistance; but bridge piers were generally made like Fig. 6. It

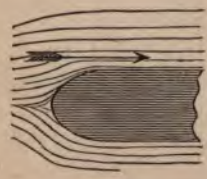


FIG. 5.

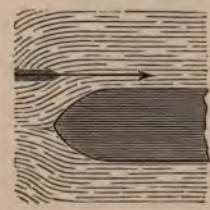
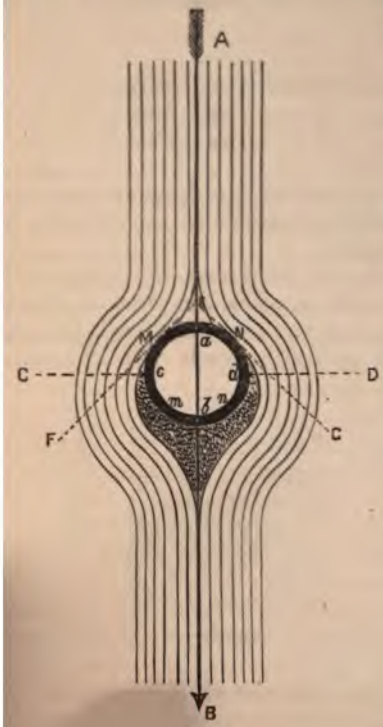


FIG. 6.

was a singular and somewhat unexpected circumstance, that the angle at which neither pressure nor vacuum existed should

FIG. 7.



G. 8.

be $41\frac{1}{2}°$ to the direction of travel, A B (Figs. 7 and 8.) Some light might be thrown on the subject by considering that in passing through the water the tube separated two streams, as M F and M G, which were driven asunder with sufficient force to prevent their clinging closely round the tube, and so at right angles to the line of direction, A B, namely, at (*c*) and (*d*) the currents were still passing away from the tube and naturally forming a partial vacuum. The point of no pressure would be that where the currents passed at right angles to the tube, so that orifices at those points neither indicated a pressure entering nor a current leaving, and experiment had shown $41\frac{1}{2}°$ to be such a point. It was another singular discovery that the vacuum should be greater at the sides (*c*) and (*d*) than at (*b*) behind, but that might be caused by the strength of the induced current away from the tube (or the minus pressure) remaining equal to that on the forward part of the tube (or the plus pressure), but acting, of course, in an opposite manner upon the water column inside the tube. That, added to the natural vacuum 2, would

give the ascertained vacuum of 6 at the sides of the tube.

Mr. PENDRED observed that there was a remarkable correspondence in all the figures on the diagram. There was 2, 4, 6, and he would ask why should those precise relations hold to one another? It was not a question of theory. With regard to the tube he had only stated the results of practice extending over twenty years, and embracing some 20,000 experiments. For several years the Berthon log had been fitted on to one of the Channel steamers, the *Courier*, running from Southampton, and every possible arrangement of the log was tested. No difference was ever made in the form of the tube, because it was necessary that it should revolve, and an oval tube would not turn round in a stuffing box. The results held good for all speeds, from the lowest speed at which it was possible to propel a steamer—keeping steerage way on her at all—to the highest speed that could be got. That was to say, it held good within all reasonable limits, from two knots an hour up to sixteen knots an hour.

Mr. BERNAYS said that in his opinion there was no better instrument for measuring the speed of ships in water than the Berthon log. But in order to obtain the distances traversed by ships, it would be necessary to make the instrument self-registering. As it was at present no distance could be calculated with any degree of certainty. The speed of a vessel was shown at any particular moment, but unless the instrument was continuously watched, it was impossible to reckon how far the vessel had travelled. The Berthon log was a very beautiful apparatus, because it involved a very simple law of nature, viz., the height varying as the square of the speed. It was, however, strange that in the first place the neutral point should always be at the same angle, and also that the two speeds—the minus and the plus speed—should always be in that exact proportion to each other.

Mr. CARGILL said that, assuming a different shaped tube or aperture was used to that at present employed, and that it showed a greater speed made by the ship, which then would be the true speed, or how would such a point be determined?

Mr. BERNAYS said he did not think an alteration in the shape would make any difference, because the point of the tube would always be at the same height, and the neutral point would therefore remain the same, so that the difference would always be the same.

Mr. COPLAND said there was another element in the construction of the apparatus that might alter the result very materially. The tube in the diagram was of a certain diameter;

the aperture at the side was also in a certain position, and had a certain definite area, and he should say that the relation which the area of that aperture at the side bore to the total area of the tube would very materially influence the mode in which it would act, and for the following reasons:—First, the aperture would not only be larger when at right angles to the course of the vessel, but when the aperture was set at the angle 41° 30′, the centre only of the aperture would bear that relation to the centre line of the tube, but the edges of the aperture would be at a different degree; therefore, the results would be rather different from those given by a larger aperture.

Mr. Rigg said that he had brought forward the experiments made with square, cubical, and prismatic bodies to show forms other than circular would probably give different results, so far as the pressure vacuum and neutral point were concerned, and even the length of tube affected those questions very greatly. The pressure in front might be indicated by the plus, and the vacuum behind by a minus sign. The front pressure of the current, when reflected from the surface of the tube and driven off from it, would become minus, and be added to the minus 2 already existing at the side and behind, thus giving a vacuum of −6 at the side and −2 behind.

Mr. Henwood said he did not think anything but the round form of tube would answer. The orifices would not answer as well in any differently shaped tube. It had been found by experiments that when the orifice was turned directly ahead it represented four inches of mercury. When it was turned the opposite way, and was pointing aft, the column of mercury went down somewhat, which was perhaps represented by a minus quantity of two, found to be midway between the two. In the same way different angles might be found. For instance, if it was put at an angle of 45° astern, no doubt it would be minus 4, and at the angle of 45° forward it would be a somewhat minus quantity. The neutral point represented by zero at the angle of 41° 30′ had been determined by experiments; and it was easy to understand that the current impinged on the fore and aft line of the keel and each portion of the current bore off against it, so that it resulted in neutral action on the point of the tube at that angle. He thought that if a tube a foot in diameter were used, instead of one ½ an inch or ⅜ inch, there would not be the least difference in the speed of the ship. He did not think the size of the orifice would have the least effect, whether large or small, because the gauge was in the column of mercury. He did not think the Berthon log was an apparatus for indicating the rate of the current; it showed when the ship was going through that current what the strength of that current was, and what extra

resistance the ship had to meet, but it could not indicate the speed of the current, as the ship generally went faster than the current.

Mr. PENDRED in reply said, Mr. Olrick's explanation of the nature of the currents, and his reason why 41° 30′ should be a neutral point, were not satisfactory. As he understood Mr. Olrick, and Mr. Rigg also, their explanations were practically identical, and were to the effect that a current was set up by the tube splitting the water, that current being precisely at right angles to the apertures, which current effectually shut off the water within the tube from the water outside. It appeared to him (Mr. Pendred) that it was totally opposed to all the known laws of induced currents, in fact, so far from what Mr. Rigg and Mr. Olrick stated being the case, if a tube was immersed in a vessel of water, and if a current of air was blown directly across it, from another tube, the water in the first tube would be raised. His (Mr. Pendred's) explanation of what took place was totally different, and it was, that no such current was set up at that point of the tube at all. His theory was that a current might be established against the tube at rest, or the tube might be moved through the water at rest. According to Mr. Rigg's explanation there was a direct impingement of particles of water on the surface of the tube. No doubt there was such a current set up as that which Mr. Rigg had shown, but he (Mr. Pendred) believed that that began in the first place, with the mere thin skin of the particles, and that they really did not get the orifice up to that point, but much further back in the tube than was previously shown. There was then a direct impingement of water at the surface of the tube which would set up a direct pressure, but, as the orifice was brought further round the tube, taking the angle directly in front, part of the body of water would turn off. That would set up an induced current on the water within the tube; and there would then be two contending forces; one would be the direction given to the water on the forward side of the aperture, and the other, that on the after side, tending to draw the water out of the tube. That was his theory, and if it was a correct one, the size of the aperture would exert a very remarkable influence on the height of the column; but in practice it had been found that within reasonable limits the size of the aperture had no influence whatever on the log. No experiments, that he was acquainted with, had been tried with any other form of tube but the circular, for reasons which Mr. Henwood had explained. The log was not constructed as a philosophical instrument for experiments, but it was a practical apparatus for measuring the speed of ships, and all the results that had been

obtained had been obtained by actual use. With reference to the question of indicating leeway which had been raised, he would explain that when a ship had leeway her motion was the resultant of two forces. Suppose the ship had the wind blowing right on her beam, that wind giving her true leeway, and she had her own propelling machinery driving her ahead, so long as the ship was moving directly parallel with her keel she had no leeway. Under such circumstances, the log, in its proper position for measuring the true speed of the ship, would be directly in the centre, and then the hole in the side of the tube was at 41° 30′; but the moment the ship got leeway on, that was, when she no longer moved in the line on her keel, that would be a different angle, and then the log would fall. The angle had then to be turned again, to get the highest point, and that would be found on the dial so many degrees out of her centre; therefore it was known that the ship was no longer moving in the direct line of her keel. Perhaps the difficulty about ascertaining leeway would not have arisen if he had stated one point which he had omitted in order to avoid confusion, but to which he would now refer. On the log, as it had been fitted in several vessels, a large vane was attached at the back of the tube, so that in practice, the ship, when moving ahead, always kept the way tube turned to the proper position, and in fact, instead of regulating the hand, the hand regulated itself, and by inspecting the dial, it was known whether the ship was moving in the exact line of keel or not. This arrangement, however, somewhat complicated the apparatus, besides which, the tube could not be replaced without the ship being put into dock, whereas without the vane the tube could be replaced easily at any time. With regard to the use of the log for registering, Mr. Berthon had a method by which the log perfectly registered itself. That was effected by photographing on a paper a register of the indications of the tube.

THE END.

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